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Notices of the Aëronautical Society of Great Britain.

Council, 1916-17.

As a result of the election at the Annual General Meeting the Council for the current year consists of:—

Associate Fellows.—A. E. Berriman, Griffith Brewer, Harris Booth, J. H. Ledeboer, Lieut. A. R. Low, R.N.V.R., Squadron-Commander F. K. McClean, R.N.A.S., Squadron-Commander Alec Ogilvie, R.N.A.S., Lieut.-Col. Mervyn O'Gorman, C.B., F. Handley Page, Dr. T. E. Stanton, F.R.S., Lieut.-Col. F. H. Sykes, C.M.G., Dr. A. P. Thurston.

Members.—Squadron-Commander G. Aldwell, R.N., Col. H. E. Rawson, C.B., R.E., Major-General R. M. Ruck, C.B., R.E., Dr. R. Mullineux Walmsley.

The Society's Activities.

The preoccupation of the Members and Staff on war work renders it necessary again to postpone the Obituary and Review Notices in the Journal.

Edward Busk Memorial.

The Edward Busk Lecture Fund is now deposited at Coutts' Bank. The trustees are Major-General R. M. Ruck, C.B., R.E., Lieut.-Col. Mervyn O'Gorman, C.B., and Mr. Griffith Brewer.

The Trustees of the Edward Busk Studentship Fund will shortly be appointed, and the studentship application forms are now in preparation.

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ANNUAL GENERAL MEETING.

The ANNUAL GENERAL MEETING of the Aëronautical Society of Great Britain was held on Tuesday, 6th June, 1916, at 2.30 p.m., at the Royal Society of Arts, John Street, Adelphi, London, W.C. The Chairman of Council, Major-General R. M. Ruck, C.B., R.E., presided.

The SECRETARY read the notice convening the meeting.

The CHAIRMAN asked that the meeting approve the postponement until that evening, owing to the war, of the date of the Annual General Meeting, which should, under Rule 11, have been held before 31st March.

The postponement was approved.

The SECRETARY read the

Council's Report, 1915-16.

The Jubilee Year of the Society.

Arrangements were in contemplation adequately to celebrate the completion of the fiftieth year of the Society's existence, but all celebrations of this event have now been postponed until after the war, when it is felt that the striking vindication of the Society's aims which the work of aircraft in the war has afforded will give the jubilee added importance.

The Society and the War.

The work of the Society has been much affected in the past year by the pre-occupation of the members and the office staff on war service. This pre-occupation, coupled with the undesirability of reading or discussing papers that might convey information to the enemy, has led to the discontinuance of the ordinary fortnightly meetings. As the office staff has been engaged, under the arrangement mentioned last year, mainly on Government work, much of the routine work of the Society has been in abeyance, and the statistics usually given in the annual report have not yet been compiled for the year under review. It should be said, however, that the number of members on active service now much exceeds the 100 mentioned last year. The honours gained and the casualties sustained by members have been proportionately great.

In Memoriam (Edward T. Busk).

The total of the Edward Busk Studentship Fund now approaches £3,000. It is hoped to inaugurate the Studentship by choosing the first student within the next few months.

Two representatives of the Society on the Committee of the Studentship have been appointed, viz. :—Mr. Horace Darwin, F.R.S., F.Aë.S., Dr. R. T. Glazebrook, C.B., F.R.S., F.Aë.S. The name of the Society's third representative will shortly be announced.

The Edward Busk Lecture Fund is now about to be formally vested in Trustees. It was anticipated that the first lecture would be given in the autumn of 1915 (by Mr. F. W. Lanchester), but the calls of the war caused a postponement until this year. The date will be announced later.

Proposed Incorporation of the Society.

In view of the present and probable future developments in Aeronautics the Council feel that the time is ripe for incorporating the Society, and they are taking steps accordingly.

Wilbur Wright Memorial Lecture, 1915.

The Third Wilbur Wright Memorial Lecture was read on 20th May, 1915, by Professor G. H. Bryan, Sc.D., F.R.S., before a large and distinguished audience.

The Fourth Lecture ("The Life and Work of Wilbur Wright") will be delivered by Mr. Griffith Brewer, A.F.Aë.S., on Tuesday, 6th June, 1916, at 3 p.m. The Right Hon. Lord Montagu of Beaulieu will preside.

The Society's Gold Medal.

The official presentation of the Gold Medals awarded respectively to Professor G. H. Bryan and Mr. Edward T. Busk took place immediately before the Wilbur Wright Lecture, 1915.

The Society of British Aircraft Constructors.

The Council have welcomed the recent formation of the new body representing the trade interests of the Aeronautical industry, and will lose no opportunity of co-operating with the new Society should occasion arise. It is a source of gratification that so many of the founders of the new Society should be long-standing members of the Aeronautical Society.

Finance.

The difficulty of keeping in touch with some members on active service has increased the total of outstanding subscriptions, but the support of the majority of the members has been continued in a most gratifying manner, and has enabled the Society's sound financial position to be not only maintained but even somewhat improved during the past year.

It is clear that a very large increase in the Society's work will come after the war, and it is confidently anticipated that all members will help in obtaining the large influx of new members which will render financially possible the desired expansion of the Society's activities.

The CHAIRMAN: You have heard the Report read and a copy of the Balance Sheet is in the hands of all members. I now ask for any remarks on the Report and Balance Sheet.

As there are no remarks I now formally put the Report and Accounts for adoption. Carried unanimously.

The CHAIRMAN: I now ask, in accordance with Rule 11, that the following retiring Members of Council be proposed for re-election for the current year, viz.:—Harris Booth, J. H. Ledeboer, Lieut. A. R. Low, R.N.V.R., Squadron-Commander F. K. McClean, Major-General R. M. Ruck, C.B., R.E., Dr. T. E. Stanton, F.R.S., Lieut.-Col. F. H. Sykes, C.M.G., Dr. R. Mullineux Walmsley. A notice asking for fresh nominations has been duly circulated, but has met with no response.

The proposition that the retiring members of Council be re-elected was made by Col. de Villamil, seconded by Mr. Riach, put and carried unanimously.

The meeting then terminated.

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THE FIRST MEETING of the FIFTY-FIRST SESSION of the Aëronautical Society of Great Britain was held at the Royal Society of Arts, John Street, Adelphi, London, W.C., on Tuesday, 6th June, 1916, at 3.0 p.m. The Right Hon. Lord Montagu of Beaulieu presided over a large and distinguished audience.

WILBUR WRIGHT MEMORIAL LECTURE, 1916.

THE CHAIRMAN: Before we ask Mr. Brewer to give his lecture, I am sure you would wish me to express my own and your sympathy with the relations of Lord Kitchener in the catastrophe which has just occurred. Lord Kitchener had the foresight, rare in this country, to see at the outbreak of the war where the war was leading and to provide for the necessities of the war. He will never be forgotten in that aspect. If he has died now at the height of his fame and power, it is at any rate a noble death, and he has died, as he would have wished to die, in the service of his country.

Returning to the subject of the lecture, I will bring to your notice the fact that most of the work of real value to mankind is done outside official circles and by private individuals, and it is nearly always opposed at its outset both by the pure scientist and by the Government department. The Wrights, in common with others of the same character and type, had that wonderful quality which we call instinct in animals and intuition in human beings. We shall hear from Mr. Griffith Brewer that they had not at their disposal at any time any very great means for looking at this matter from a purely scientific point of view. That has been so in other cases where inventions and improvements have occurred which have altered the face of the world and the destiny of man. For example, Trevethick and Robert Stephenson in connection with the steam engine, Otto and Daimler in connection with the internal combustion engine, Lilienthal and the Wrights in connection with flying, are instances that spring readily to the mind.

About the same time as the Wrights there was the instance of Mr. Marconi, the distinguished Italian, who, adapting to commercial use the ideas of Hertz, made wireless telegraphy the faithful servant of mankind.

The Wrights were inspired by the single idea "how to fly," and they succeeded where many failed. The history of their life and work will be read to you to-night.

THE LIFE AND WORK OF WILBUR WRIGHT.

(Being the Fourth Wilbur Wright Memorial Lecture.)

BY GRIFFITH BREWER, A.F.Aë.S.

The Wilbur Wright Lecture this year is not an addition to the store of science, but is merely a tribute to that great addition contributed by Wilbur Wright, which gave to man the third and greatest highway for intercommunication.

This exception to the generally scientific character of the lecture has been considered desirable, because the publication of the results of new research at the present time would not be wise, and also the opportunity is favourable to give to the members of the Aëronautical Society a more accurate picture of the life and work of Wilbur Wright than they may be able to gather from casual observations which have appeared from time to time in various publications.

Wilbur Wright was born eight miles due east of Newcastle, Indiana, on the 16th April, 1867. His brother Orville was born on the 19th August, 1871, after the Wright family had moved to Dayton, Ohio, in the meantime. Wilbur and Orville grew up with their elder brothers, Reuchlin and Lorin, and their younger sister Katherine, and they were fortunate in having a broad-minded father who encouraged them to expand their thoughts, and did not place obstacles in the way

FIG. 1. Wilbur Wright. Born April 16th, 1867. Died May 30th, 1912.

FIG. 2. Wilbur Wright, about 16 years old.

of their natural development. Bishop Milton Wright originally intended his son Wilbur to follow in his footsteps and enter the church, but although Wilbur had considerable literary talent, his inclinations and somewhat weak health drew him toward more active pursuits.

The city of Dayton, now a town of 120,000 inhabitants, is divided by the river Miami, and the portion of the town situated on the west of the river is known as the "West Side." The business quarter of Dayton is contained in the "East Side," the west being more used for residential purposes, but the inhabitants of the West Side take a local pride in the progress of their portion of the city, and have striven hard to import similar prosperity to the west to that enjoyed by the east.

Early Journalistic Work.

Wilbur and Orville, imbued with this local pride, commenced their activities in the West Side, and one of the earliest efforts made by Orville was in the running of a printing press. He was a boy in his teens at the time, but this did not deter him from starting a printing business, which was only limited by the smallness of the machine at his disposal and by the small quantity of type. This first printing machine is recalled by the neighbours in West Side for the ingenuity of the makeshift contrivances of bits of string and pieces of wood, with which it was made to run in a businesslike manner, and when it broke down from any cause, the magic touch of its owner readily put things right. There seemed to be a bond of sympathy between Orville and his first printing machine, for a glance from him seemed enough to set it running again, when the efforts of the printing staff had failed. It was on this first printing machine that Orville printed his first boys' paper, which charmed the youth of the neighbourhood and helped to enthuse the boy army of West Side, of which he was the Captain. Later he obtained a more efficient printing outfit, and on the 1st March, 1889, while still only seventeen years old, he launched his first weekly newspaper under the title of the "West Side News." For the first three months Orville was editor and publisher combined, of the little four-page weekly, and then the size of the paper

WEST SIDE NEWS.

Vol. 1. DAYTON, OHIO, MAY 25, 1889. No. 12

West Side News.

PUBLISHED WEEKLY.

WilburWright - - - - Editor
Orville Wright - - - - Publisher

TERMS:—Quarter of year, twenty cents Six weeks, ten cents.

1210 WEST THIRD STREET.
DAYTON, OHIO.

Ancient Warriors.

Before the invention of gunpowder a battle was little better than a group of hand-to-hand conflicts. Stout sinews and muscles were then valued, and the strong man was especially esteemed. William Wallace was the Scottish chief, not only for his patriotism but also because he had a giant's strength. Once, when attacked by five men, he killed th- and put [illegible] bough

Parrot Chorus.

The traditional "fish story" has many varieties, to which it seems only fair to add the following, even though the fish in this case was a parrot. Doubtless its narrator, an American artist, designed it to be "taken for what it is worth."

He was very fond of knocking about in out-of-the-way quarters of the world, and once left ship with a ship of comrades, in order to explore a Central American wilderness. During the cruise of several months, the entire ship's company had devoted their leisure hours to singing to a parrot. The sailor had also lost no opportunity of teaching the bird all the nautical phrases they knew.

When the artist and his comrades had bidden the bird and the sailors good-by, they plunged into the heart of the tropical forest, and after great exertion in accomplish-

forsaking all others, cleave only unto her, so long as you both shall live?"

The minister paused for the response. The groom hung down his head, and was silent, but the bride, in a staccato tone, exclaimed, "Yes, sir, I'll see to it that he does all that!

It was evident who would rule in that household. But a Scotch clergyman once married a groom who insisted upon promising to obey his wife. The clergyman, while traveling through a village, was requested to officiate at a marriage, in the absence of the parish minister. Just as he had told the bridegroom to love and honor his wife, the man interjected the words, "and obey." The clergyman, surprised to find a husband willing to take a promise usually made by the wife alone, did not heed the proposed amendment.

He was going on with the ser- [illegible] inter-

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THE
WEST SID-
Building A-

FIG. 3. Portion of first page of the "West Side News," showing Wilbur Wright's name as Editor.

was increased and Wilbur joined his brother in the capacity of editor, Orville remaining the publisher. In April, 1890, Orville and Lorin started an evening newspaper under the title "The Evening Item," but this enterprise was evidently too large for their resources, for after a run of four months it ceased to appear. In October, 1894, Wilbur and Orville Wright commenced the publication of a

SNAP-SHOTS

AT CURRENT EVENTS

VOL. I. DAYTON, OHIO, NOVEMBER 17, 1894. NO. 5.

THE

West Side Building & Loan Co.

WALTERS' BLOCK.

1122 W. Third St. *Telephone 1115*

Pays 7 per cent. dividend on all money deposited from date.

Money loaned only on first mortgage security.

Capital Stock, . . $2,000,000 00
Assets, July 1, '94 . $229,579.46
Surplus, ... $3,502.45

S. L. HERR,
President

J. C. PATTERSON,
Sec'y & Atty.

Miss Marguerite Murray

1016 WEST THIRD ST.

Fashionable Millinery Patterns,

All the Latest Novelties.

Trimming and Trimmings A Specialty.

Prices the very lowest.

Kindly favor me with a call.

FIG. 3A. Outside cover of "Snap-Shots" (actual size).

little weekly magazine, entitled, " Snap-Shots," in which Wilbur Wright contributed an article on local affairs in the form of an imaginary discussion supposed to have taken place at the Idlers' Club. Those members who knew Wilbur Wright in France will remember his quiet sarcasm with some of the scientists who came to Le Mans to criticise the aeroplane which they did not understand, and they will no doubt appreciate the humour and the courage of his writings.

Wilbur was a fearless critic, and he wrote on matters of local interest in a kindly but vigorous manner which did much to maintain the healthy public municipal life of the town of his childhood, and I was fortunate, on my last visit to Dayton, in persuading Orville and Lorin to let me bring back to England some of the precious copies of these papers containing examples of Wilbur's early journalistic work. I have chosen one article written by Wilbur, and published in the Wrights' paper " Snap-Shots " on the 17th November, 1894, as being typical of his writings at that time, and showing how his wit and wisdom were used to defend a critic who had ventured to criticise the action of four members of a local authority who were bringing four actions for libel against the local critic of their public work. The article in question is in the form of the weekly report of the proceedings of an imaginary body termed " The Idlers' Club," and is reproduced in the Appendix to this lecture.

The Wright Cycle Co.

These literary efforts and the attendant business of printing were carried on in the building at the corner of Third and Williams Streets, Dayton. Afterwards Wilbur and Orville Wright formed the Wright Cycle Co., at 1127, West Third Street, on the other side of the road to that of the printing and publishing business, and there as cycle manufacturers they produced the " Van Cleve " bicycle, a machine which earned great local repute for its excellence of construction, a reputation which lives years after the machine itself has ceased to be manufactured. The adoption of the name of " Van Cleve " for the bicycle manufactured by the Wright Cycle Co. is interesting from the fact that it was named after their ancestor, Catherine Van Cleve, who was one of the first settlers to land at Dayton from a boat on the river Miami on the 1st April, 1796, at a time when the country was virgin forest, inhabited by none but Indians. Catherine's daughter Margaret had a daughter Catherine, who married Dan Wright the younger on February 12th, 1818. Milton Wright (father of Wilbur and Orville) was born to them on the 17th November, 1828, and was fourth son and fifth child.

It is interesting to notice how loyal the Wright Brothers were to Dayton, and especially to the West Side where they lived. No doubt the business portion of the town would have proved more profitable to them, when starting one of their newspapers, or when starting their bicycle business. Then when they invented their great invention, which was one of world wide interest, one would have thought that there must be some other town in America better suited for the manufacture of the flying machine. Whether they were prompted to bring the new industry to their native town, simply because it was their home, or whether they recognised Dayton as a town with exceptional business possibilities, will probably never be known; but the fact remains that they did choose their home town for the new manufacture and they also invested some considerable portion of the fruits of the invention in improvements in the West Side.

The Brothers' First Aeronautical Work.

Although Wilbur and Orville had from their early childhood taken a keen interest in the possibilities of human flight, it was not until 1896 that they gave more than passing attention to the subject of flying. It was then that they took up the problem seriously, and studied with great interest the aeronautical information then available. Lilienthal's work and writings especially attracted them, and the fact that a man of Langley's great reputation had declared his belief in

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the possibility of human flight gave them confidence in attacking the problem which classed all who attempted it with lunatics and believers in perpetual motion. Wilbur Wright was never tired of praising the work of Lilienthal and of that great pioneer of the aeroplane, James Stringfellow, one of the founder members of the Aëronautical Society. At the banquet given on the 4th May, 1909, by the Royal Aero Club to the Wright Brothers on their return to America after Wilbur's demonstration flights in France, Germany and Italy, in 1908-9, many of you will remember the tribute he paid in his speech to the great pioneer work of Stringfellow. He reminded us how our founder member had devised an aeroplane having two propellers, a vertical rudder and a horizontal tail, and he made us realise how nearly we had approached the solution of the great problem. We recognised as he spoke that Stringfellow's aeroplane, had it been controlled and propelled with modern means and knowledge, would have been capable of sustained free flight. But we also recognised that these apparently little things were the things that count, for they were the links between the problem and the art. The very fact that the bridging of the gap had been repeatedly attempted in the intervening time without success had made the problem more forbidding and one that required more courage to attack as each experimenter entered and retired in his turn.

And so the Aëronautical Society of Great Britain, the oldest society concerned with aviation in the world, has weighed the valued work of all the pioneers, and it has never withheld the claim of America to the raising of the aeroplane from the problem to the art.

Neither Wilbur nor his brother Orville were the men to rest satisfied with theory or with paper experiments. They were firm believers in proving everything by practical experiment, and so the problem of flight was now attacked in a manner it had never been attacked before.

Never in the history of the world had men studied the problem with such scientific skill nor with such undaunted courage. They first studied all the books, then tested the data of Lilienthal and other authorities by building man-carrying gliders and risking their lives to find the truth which could not all be found by study at the fireside. Having found after two years' outdoor experiments in the air that the very axioms on which the art of flight reclined were, some correct, some inaccurate, and some absolutely false, they commenced a long series of laboratory experiments, and in the most scientific manner made thousands of tests with models of various forms of curved planes in a wind-tunnel, and tabulated thousands of readings, which even to-day, now fourteen years since, give equally efficient results to the more elaborate tables since prepared by learned institutions.

I have examined this wind tunnel myself in the stable where it was stored two years ago, and have handled the little curved planes which Orville kept in one of the drawers of his desk, and I could not help coveting these treasures for this Society to honour and preserve. But this is a confession which I have never ventured to express, although it is possible Orville may have guessed it.

I was fortunate during my last visit to Dayton, in having access to the diaries kept by Wilbur and Orville throughout their Kitty Hawk experiments, and the following portion of my paper was written partly from these diaries and partly from our conversations in the summer evenings of June, July and August, 1914.

The First Glider.

In the summer of 1900, having ascertained from the Weather Bureau at Washington that the North Carolina coast had the strongest and the most constant winds, and the Postmaster at Kitty Hawk having given the neighbouring sand dunes a good testimonial as to their roundness and softness for boys playing with flying machines, Wilbur and Orville Wright transported the various parts of the glider which they had designed and partly built at their

home at Dayton, assembled and completed building it in the tent which they brought with them. This first trip to Kitty Hawk occupied them nearly a week on the journey. Afterwards, when they knew the way better, the journey could be accomplished in three or four days. A tent was used on this first visit, and in following years they built a shed for housing the machine and for living in. They did not find the shed and household conveniences ready to receive them, but these were laid out on the sands, in the form of lumber and corrugated iron, which required sawing up and putting together, in order to afford the comfort and joys of a Robinson Crusoe existence.

The gliding experiments were carried out by launching the machine down one of the Kill Devil Hills, and gliding against the wind, the operator lying face downward on the machine and grasping a bar capable of receiving a twisting motion, which enabled the man, by twisting his outstretched hands, to guide the machine up or down when elevating or depressing the forward elevator. The wing curves of this first gliding machine were designed by the Wright Brothers on their own lines, or as Orville said, "by guess," but the data of lift, "drift," resistance of struts and framework, were taken from the tables previously published by Lilienthal.

The Second Glider.

In 1901, the Wrights went to Kitty Hawk a second time, and on this occasion with a glider having a wing surface of 290 square feet, and weighing 98lbs. This machine was believed to be an improvement on the original machine of the year before, having been designed on curves supplied to them by Mr. Chanute from Lilienthal's wing curves. This machine proved less efficient than the earlier machine. Nothing in the curve particulars supplied by Mr. Chanute mentioned that changes in curvature would alter the lift, nor that change in aspect ratio would alter the efficiency of a wing surface. The wings of the new machine were 22 feet span and 7 feet chord, and the horizontal elevator, at the front of the machine, was 7 feet distant from the front edge of the wings, and had an area of 18 sq. feet.

After making about 17 glides on the 27th July, it was found that the machine was less manageable than had been expected, the elevator being, as they imagined at the time, too large, and this was afterwards reduced to an area of 10 sq. feet. The best of the above glides was one of 315 feet in a descent of 1 in 6, time about 19 seconds, relative speed about 25 miles per hour in a wind of about 13 miles. The resistance of the framing was apparently more than had been expected from the Lilienthal tables and doubts arose as to the correctness in the tables of the ratio of lift to "drift." Later, the machine was flown as a kite, but the lift obtained was found to be much less than the tables led them to expect, the amount of lift obtained being only one-third as much as it should have been. They had intended to experiment most of the time with the machine in an 18 miles an hour wind without much forward motion being necessary, so the poor lift of only one-third of that anticipated was very discouraging, because it would be necessary to glide in order to attain a sustaining speed through the air. The larger machine used in the experiments in 1901 was found to be less manageable than the smaller machine used the previous year, and this trouble was at the time attributed to the travel of the centre of pressure.

The Wind Tunnel Experiments.

On returning to Dayton in August, 1901, the Wrights decided to check the data given by Lilienthal by making independent experiments of their own, and they very soon found many discrepancies which filled them with interest and led them on to make experiments more thorough and complete than any scientific experiments on the subject hitherto made. They constructed their wind tunnel 16 inches square, inside measurement, and about six feet long, into one end

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of which a current of air was blown by a fan, and the direction of the draught was straightened by a series of "pigeon holes." Beyond these wind straighteners pivoted arms were arranged, carrying the small model planes and wings to be tested, and more than two hundred miniature wings were tried in this wind tunnel to ascertain their lift and "drift." Many of these models were made to the same proportional curves but to different scales, in order to verify the results and to ascertain what difference, beyond that of proportion, change in scale might entail. Each model was tested at angles of 0°, 2½°, 5°, 7½°, 10°, 12½°, 15°, 17½°, 20°, 25°, 30°, 35°, 40°, 45°, and some thousands of readings were taken and tabulated, and it is on these readings that the subsequent work of the Wright Brothers has been based. Most of the models were made with an aspect ratio of 1-1, 1-4, and 1-6, and a few 1-2, 1-8 and 1-12. This was the first time that any systematic measurements had ever been taken to ascertain what relation aspect ratio had on various types of wing surfaces. Never before, so far as then known, had the relative lift of oblong surfaces been taken in comparison with square surfaces. These tables compiled by the Wrights have not yet been published, but they have been in constant use by the Wrights ever since. The tables of figures are contained in long pocket books, and members of this Society who have been with either of the brothers at any of their experimental flights will have observed them refer to these precious little volumes when making a calculation. The models tested in this way are made of sheet metal, mostly bent over centrally lengthwise, the bent-over portion forming the bluff leading edge of the required curve, the free edges for the plates being brought back together, one slightly in advance of the other, and after being joined being smoothed off with wax. Other surfaces tested are of a single thickness of sheet metal and curved to varying degrees, while still others are bent to arcs of circles.

The result of these experiments was the abandonment of the tables of earlier experimenters which the Wrights had previously used, and the third machine built for the purpose of the continuation of the experiments at Kitty Hawk, proved by the efficiency of its gliding angle, that they were fully justified in going to the labour and expense of this independent scientific work.

The Third Glider.

On August 25th, 1902, Wilbur and Orville Wright again left Dayton for Kitty Hawk in order to resume the outdoor experiments they had commenced in the two previous years. They had learnt much by the wind tunnel experiments which they had made during the winter before, and these tests in the wind tunnel enabled them to design and build the third glider of considerably greater efficiency than the two earlier gliders built to the old formula. Being experimenters of a practical turn of mind, they realised how necessary it was to test by practice in the air itself the truth or falsity of apparent answers to the many problems. The aspect ratio of this new machine was one to six, instead of about one to three as in the second machine, this change being decided on in view of the knowledge gained in the wind tunnel experiments made during the previous winter. The journey to their experimental ground was arduous enough to have daunted many who were less enthusiastic, for it first involved, if lucky in making train connections, 25 hours rail journey to Elizabeth City, thence by sailing vessel to Dosher's Wharf, taking anything up to another 24 hours, and then by a small boat to the camp, situated four miles or so from Kitty Hawk. After arrival in camp, the remnants of the shed left by the winter gales had to be put into order, living arrangements put right, such as arranging the kitchen and driving a 16ft. well, and so the first days were devoted to hard work in order to support existence before any thoughts of flying experiments could be indulged in.

On this third visit to Kitty Hawk, therefore, although the brothers left their home on August 25th, 1902, it was not until September 8th that they were able even to commence work on the erection and construction of the glider. On

September 10th the upper wings were ready, and a test of these was made for efficiency. It was found that the covering of the rear side of the rear spar made the centre of pressure reverse sooner than when left exposed. On September 13th the lower wings were finished. On September 16th the struts and connections of the upper and lower wings were completed sufficiently to enable the partly finished machine to be again tested, and on September 19th the machine was completed ready for making free glides. The above will give some idea of the work and discomfort that had to be gone through before any actual experiments could even be commenced, and after doing all this the very first experiment might bring a smash entailing at the least the recommencement of the constructional work, or better still, the discovery of some error, which although entailing tedious alterations, would weed out one more snag from the road to human flight.

Both Wilbur and Orville Wright made careful observation of the flight of birds. The buzzard, which frequents North Carolina and uses the strong up currents rising from the heated sand and deflected upwards by the sand dunes, was often observed with special interest. On one occasion some buzzards were observed soaring over the sand hills. The conditions appeared to be such that they were unable to soar over plains, but they took to the hills where they had considerable trouble in gaining altitude of more than 50 to 75 feet above the top of the large hill. Orville describes how they watched the buzzards with field glasses at a distance of 1,200 feet. The angle of the birds above the horizon was from $4\frac{1}{2}°$ to $5\frac{1}{2}°$ taken with a clinometer. They could see the underside of the wings all the time when the birds faced them, being directly in line with the wind from them, and the upper side of the wings could be seen when the birds soared away from them. The velocity of the wind was from 6 to 8 metres per second, and the slope of the hill $12\frac{1}{2}°$.

The third machine, which was completed on September 19th, contained 305 sq. ft. of surface on the upper and lower wings, the front elevator contained 15 sq. ft., and two vertical surfaces $11\frac{2}{3}$ sq. ft. The same afternoon about 25 glides were made and the front elevator was found to give ample longitudinal control with a change of not more than two or three degrees to either side of the central position of 0°.

Successful Glides.

The following afternoon several glides were made down the large hill, Wilbur making one of these glides in 11 seconds, covering a distance a little over 200 ft., in which the machine began to gradually slide towards the side of the right wing, which was the lower one. The flight was made a little to the right of the direction of the wind, and the wind getting under the left wing from the side, raised it higher and higher, when suddenly by mistake, while attempting to alter the wing tips, he turned the front elevator down at the rear, causing the machine to assume an angle apparently of over 45°. In the descent from this position, the height of the left wing became still higher as compared with the right, and the machine made a fast downward plunge directly towards the right wing. Before striking the ground, the torsion of the wings brought the left wing down considerably, and the machine running almost directly at right angles with the wind, struck on the right wing and swung round with the wind blowing from the rear.

Later in the same afternoon a number of tests of the torsional effect were made by running with the machine down a 7° slope on the smallest sand hill, and it was found that the wing with the larger angle, though started with the wind blowing from the other side of the machine and under the opposite wing and sidling off, would swing around the lower wing having the smaller angle. The afternoon's experiments were concluded with a glide made by Wilbur 140ft. down the north-east slope of the small hill with an angle of descent of $7\frac{1}{6}°$. Wind velocity at place of start $5\frac{1}{6}$ metres per second and at place of landing $4\frac{1}{6}$ metres.

XUM

The relative wind was estimated at 18 to 20 miles per hour, this estimate being based on a run made by Orville down the hill over the course of the glide with the anemometer and watch, in which the anemometer recorded 69 metres and the watch $6^3/_5$ seconds. His speed in running was probably greater than that attained by the machine at any time by 2 or 3 miles.

As a result of the above experiments, the truss wires were altered so as to give an arch to the wings, making the ends four inches lower than the centre, and the angle at the tips greater than at the centre. The machine was then flown as a kite with very satisfactory results. By these alterations, the trouble experienced heretofore with a cross wind turning up the wing it had first struck had been overcome, and these trials seemed to indicate that the arching of the surfaces laterally produced the opposite effect. The machine now flew beautifully, and at times, when the proper angle of incidence was attained, seemed to soar, although the angle of the hill was only a little over $6\frac{1}{2}°$.

In some of the glides made afterwards by Wilbur, he remained stationary in the air, without descending the hill or losing his altitude from the ground for periods of four or five seconds at a time, or until he was compelled to descend for safety, when the angles of the wings needed changing, these being fastened stationary during these glides. Orville also made his first free flights that season, gliding on the N.N.W. slope of the big Kill Devil Hill, where the slope at its greatest was $9\frac{1}{2}°$. After about half a dozen attempts, he made a glide of about 160 ft. with a total angle of descent of 5° 57′. Soon after Wilbur began to use the end controls and found that with the changes made the day before they worked perfectly, or rather as Orville observed, they would work perfectly when they had learnt to manipulate them properly. It was found, however, that the machine still had the fault of suddenly turning up further and further in front when the wind got under one wing, unless the front elevator was turned a little downward in order to overcome this effect. Wilbur made a number of glides, in which he descended from almost a standstill in the air, with one end down, landing however on one wing with no damage to the machine. On one of the glides made by Orville, from a distance of 175 ft. above the bottom of the hill, the machine turned up steeply in front, lost all headway, and with one of the wings up at a large angle descended for 20 or 25 ft. backwards, and finally landed going forward with one wing up at an angle which appeared to be nearly 45°, but was probably less than that. It speaks volumes for the strength of the structure that no damage was done to the machine.

A "Smash."

The last glide made by Orville ended in disaster. He was flying the machine with the control loose, so that he could adjust it during flight, when he noticed that one wing was gradually getting a little too high and the machine was slowly "sidling" off toward the low wing. He thought that by moving the end control mechanism an inch or so he would bring the wing back to its proper position. The next thing he knew, the wing was very high in the air, a great deal higher than before, and he thought he must have moved the side control in the wrong direction. Thinking of nothing but the side control, after reassuring himself of the proper direction, he threw the wing tips to their greatest angle. By this time he found that the machine was descending backwards towards the lower wing, from a height of 25 or 30 feet, as a result of the machine having turned up at an angle of nearly 45° in front, which fact he had not noticed while occupied in manipulating the controls of the wing tips, but which had been witnessed by Wilbur and by Dan Tate,* with alarm, for several seconds before. The result of this experimental glide was a heap of flying machine, cloth and sticks in a pile, with Orville in the middle without a bruise or scratch of any description.

* See Appendix B., page 86.

This adventure terminated the experiments for the day, and surprising as it may seem, it left them in the highest spirits, for the encouraging performances of the machine, both in control and in the angles of flight, far outweighed the work that would be entailed in repairing the machine. It is small wonder that they should be pleased at the result of the experiments, seeing that the machine glided at least 3° better than any glider had ever glided before, and they had gained the valuable information that a machine with a front elevator is liable to tilt upwards when the relative wind is from one side, owing to the wind striking on the under side of the front elevator. It is evident that only extreme optimists could succeed in experiments of this kind, optimists with a steadfast resolution to carry them over the many obstacles, each of which once surmounted left one more snag behind without disclosing the number still ahead.

The next illustrations relate to Wilbur's life at Kitty Hawk during the power flight experiments.

Fig. 8 shows how isolated was the Camp at Kitty Hawk, although the kitchen shown in Fig. 9 is a fine example of the methodical way in which the Wright Brothers did everything.

The next two illustrations show the power machine which the Wrights always referred to as a "flyer," as distinct from "gliders," which have no power beyond gravity with which to drive them.

Fig. 12 is especially interesting because it shows the first instance of a flyer leaving the ground on a controlled flight. Orville is seen lying on the machine and Wilbur is seen to the right. A piece of one of the struts of this identical machine is on the table, and can be examined after the lecture.

The remaining five illustrations show the continuation of the gliding experiments, carried out by Orville Wright and Alec Ogilvie in 1911.

Figs. 13 and 14 show glides with Ogilvie up.

Fig. 15 shows the arduous work of getting back to the top of the hill.

Fig. 16 is now historic. It shows the beating of all records for soaring flight, accomplished by Orville Wright, who remained balancing the machine in an up-current for ten minutes.

Fig. 17 illustrates one of the many narrow escapes inseparable from successful gliding experiments.

Wilbur Wright was a master in all his work. I found my knowledge of inventions and of patent matters open to continual improvement in keen discussions with him on the subject, from the inventor's point of view. His deep knowledge on the many subjects we discussed, and his clearly defined arguments, gave me a new outlook on many things from which I have profited ever since. And so in presenting Wilbur Wright's writings, which form the appendix to this lecture, to his fellow journalists, I do so with confidence, knowing they will recognise that literary talent which would have taken him far with his pen had the air not claimed him for greater work.

Appendix A. has already been referred to, and in reading that little article some insight may be gathered of the personality of Wilbur Wright before he attacked the flying problem. It takes courage as well as wit to write thus of public men.

Appendix B. most of you have read, some of you more than once. It will stand reading a third time, for it contains many lessons, from which some of our members may still be able to profit.

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Appendix C. consists of a letter written by Mr. Harvey M. Weaver to Mr. Lahm, in Paris, giving an excellent account by an astute observer of the performances of the "Wright Flyer" at the end of 1905. It was a well-known member of the Aëronautical Society, Mr. Patrick Alexander, who brought the news to Europe and who told Mr. Lahm at a balloon contest at Paris that the Wrights had flown two and a half miles. Mr. Lahm received the news with caution, and later, on December 1st, 1905, he cabled to his friend, Mr. Weaver, in America, with the result of receiving this now historic letter which forms Appendix C. to this Lecture. The letter, as you will see when you read it later, placed the question of the flights being actually made beyond all doubt, but although it convinced Mr. Lahm, it did not convince the Aero Club de France.

A translation of this letter was read by Mr. Lahm before the Aviation Committee of the Aero Club de France on December 29th, 1905, presided over on that day by Mr. Ernest Archdeacon, when it was discussed and criticised severely. Doubts were thrown on Mr. Lahm's news, not because anyone doubted him or his friend in America, but it was believed that flight was impossible with a motor of 12 to 15 horse-power. Moreover, many prominent members of the French Aero Club had begun to believe that mechanical flight was an impossibility, and so the first news of the accomplishment of the impossible was even harder for them to believe than for those who had not tried and failed. Mr. Santos Dumont, who was present at the Aero Club de France when Mr. Lahm read his astounding news, had not up to that time given any outward sign of work in mechanical flight as distinct from extensive dirigible balloon experiments.

It therefore came as a surprise when, on January 2, 1906, Mr. Santos Dumont inscribed his name as a competitor for the Deutsch-Archdeacon Aviation Prize. We all know how pluckily he strove to win the distinction of being the first to fly in France, and how in the autumn of 1906 he won the prize on an aeroplane of his own construction. The fact that he attempted to construct a helicopter before he adopted the aeroplane system does not detract from the merit of that strenuous year, when, in addition to his experiments in pure aviation, he competed, with a combined balloon and helicopter, in the Gordon Bennett Balloon Race from Paris in October, 1906.

Considerable information about Wilbur Wright's work has been published by this Society from time to time in THE AERONAUTICAL JOURNAL, and students of the history of aviation should refer to the following portions of the Journal:—January, 1906, pages 14-15; April, 1906, pages 25-26; July, 1906, pages 37-38; July, 1912, page 148.*

Appendix D. is the reprint from the article in the "Century Magazine," September, 1908, which made that popular magazine doubly welcome to all interested in aviation, for it came to England at the time Wilbur Wright was engaged at Le Mans showing all who cared to visit him how the thing was done. This article was written jointly by Orville and Wilbur Wright, and it needs no bush.

Appendix E. is a fine example of Wilbur Wright's lucid argument. His tribute to Mouillard as a missionary of aviation and profound observer of the Vulture's soaring flights, and his defence of Chanute, who had been accused of betraying Mouillard's secrets, is a good example of Wilbur's skill in selecting the crucial points in a much involved discussion debated in the Press some five years ago.

Appendix F. is another forcible argument from the pen of Wilbur Wright. This was written to refute the attempt being made to rob America of the glory of being the birthplace of human flight. He tells us how some misguided people first hailed Santos Dumont as the "father of aviation" because of his

* These numbers being either out of print or very rare, the extracts are reprinted as Appendices H to K herein.

flights in 1906. Then when it was proved that flights had been made in America before that time they fell back on Ader on account of a mythical flight in 1897. The mythical flight was published broadcast, and British writers of recent books on aviation still quote Ader on the strength of this fable as the first to fly. This article, quoting General Mensier's official Report of the alleged flights, showing that Ader's machine did not leave the ground, should therefore be of value to British authors in the cause of correctness in the history of aviation.

Appendix G. is the evidence given by Wilbur Wright in the Patent Action in America, now published for the first time, and will be read with special interest by those who have followed the legal battle.

Both Wilbur and Orville were never so well in health as when conducting their experiments in the open air, and they both succeeded in their gliding adventures without serious mishap. The legal work, which under present Patent Laws is thrown on all successful inventors, proved a far heavier task, and rather than let this work fall below the usual standard of all his undertakings, Wilbur overtaxed his strength and reduced his vitality by overwork till he became an easy prey to his fatal illness.

To my mind, the most fascinating portion of Wilbur Wright's work was during that first three years, when he approached the flying problem as a sport and recreation. Alone, he could not have succeeded. Alone, Orville would have failed. But their sporting instinct and boyish love of adventure gave each the energy and will to support his brother's efforts, neither striving to be first, both working to one end. The sport of gliding, with life in the open air, was the first attraction, then the interest derived from the series of wind tunnel experiments during the winter between the outdoor sport, then the gradual combining of the sport with the science, when the determination of two master minds fought trial after trial, until at last they learnt the art of flight upon the steed of their own creation. There was no question of a light power being the key to flight, because the light power had been produced years before by Henson, Maxim, and by Langley, and the Wrights before they made their engine had learnt to fly and had built the machine to fly with. So certain is this view of the situation that during the winter after the end of the gliding experiments they designed and built their first power flying machine, and in the autumn of the following year they took it to their familiar sand hills and flew it at the very first attempt. The details of these further flights on the power flyers you should read in the words of Wilbur Wright which are collected and published in the Appendix to this lecture.

The Alleged Mystery and Secrecy of the Brothers' Work.

It has often been said that the Wrights made a mystery of their experiments and worked with great secrecy. This is wholly opposed to their real characters. They naturally desired to experiment quietly in an out of the way corner, because all who tried to fly were laughed at, and continual jeering is apt to detract from useful thought. Afterwards, when people recognised that their work was sane and possibly the greatest advance in science for a century or more, they were still obliged to seek seclusion in order to be able to work at all. Outside of work time, however, none have had their questions left unanswered, and those who knew Wilbur in France will remember his patience in giving information to all who sought it.

The Patent Position.

There is one aspect of the Wright Brothers' work I cannot help regretting; an aspect which might have been averted had their work been recognised before it triumphed by sheer force of right. So long as it was merely a sport the

Wrights had no thought of profit, but when they attacked the problem more seriously and gave up their livelihood to win through on that uphill fight, they were obliged to do what all inventors do, and seek to make their invention provide the cost of its production.

America had already paid some public funds to find the road to flight. France had done the same with no more success to show in return. The Wrights working silently in the wilderness, and relying entirely on their own resources, quietly emerged with a machine which could fly miles without mishap. Everyone cheered and welcomed the advent of the hope of ages.

It was not until people realised that the invention was expected to produce its reward to the inventors that men began to murmur, and then after one triumphant year in Europe and their return as conquering heroes to their native land, did we hear rumours of defiance and refusal to pay tribute for the use of the patent. A great industry came into being, giving employment to tens of thousands, but after the first year or so these new vehicles of transit ceased to pay royalties to the inventors. The invention was too big to pay in silence, and the law too ready to exploit the work of the inventors.

It must therefore be a matter of considerable satisfaction to my fellow countrymen that the use of the aeroplane in Great Britain has not formed the subject of litigation between the inventors and the manufacturers. The country has had the full advantage of the invention without the restraining effect of threats. No licence has ever been refused under the British Wright Patents, and at the present time no impediment exists for the free use of the invention for manufacturers to make machines for the British Army or the Navy, which is in fact the only real market for aeroplanes in Great Britain.

At the same time, this free privilege of the British manufacturers, unique in its position amongst the great manufacturing nations, was not entirely a free gift. It was felt by the British supporters of the Wright Brothers that recognition of their pioneer work was necessary, if only to justify the patenting of the invention. That which costs nothing is generally worth nothing, and so it was necessary to draw the orthodox tribute.

The Action Against H.M. Government.

The question how to do this with the least disturbance to the trade was carefully considered, and it was decided to apply to the Government for leave to sue a nominee of His Majesty for infringement. This permission was graciously accorded, and an action was brought for infringement of the Wright patents. This action was commenced early in 1913 and the trial would have been heard in the autumn of 1914. Then the war came. I was in America with Orville at the time, and I hurried back to England along with the rush home of Englishmen all the world over. A meeting of the Wright supporters in England unanimously agreed that however friendly the action against the Government was, we could not reasonably continue to fight the Government during such a time, and so we offered to accept the smallest sum compatible with real recognition, viz., £15,000, for immediate settlement. This offer was promptly accepted, giving thereby official recognition of the validity of the Wrights' claim, and giving to the nation complete freedom to use, for its defence, the invention which enables it to hold its own in the air. Germany and Austria also use the invention, but they do so without the satisfaction of having paid for it.

I have gone to this length in telling you of the patent position in Great Britain, because so many rumours have appeared which would imply a lack of generosity on Wilbur's part, and I hope you will excuse this reference to the part I felt called upon to play, after Wilbur was no longer here to play it. My endeavour has been to do what I believe he would have done, and I thank those who so well

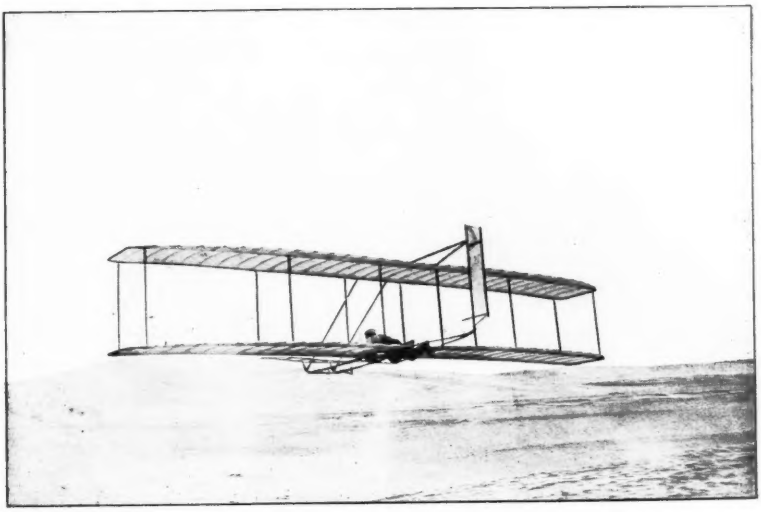


FIG. 4. One of the gliding machines of 1902.

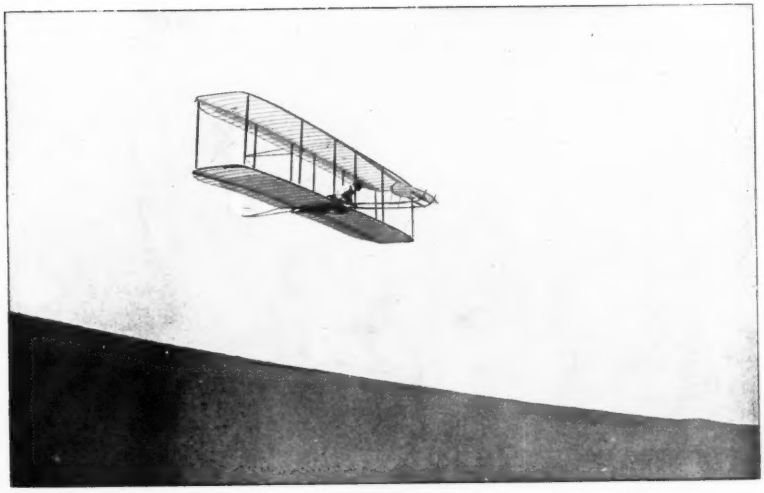


FIG. 5. A high glide.

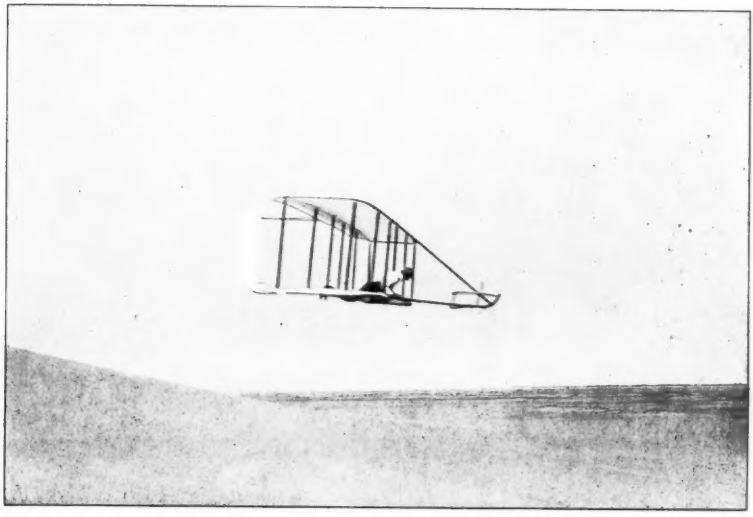


FIG. 6. Wilbur Wright gliding.

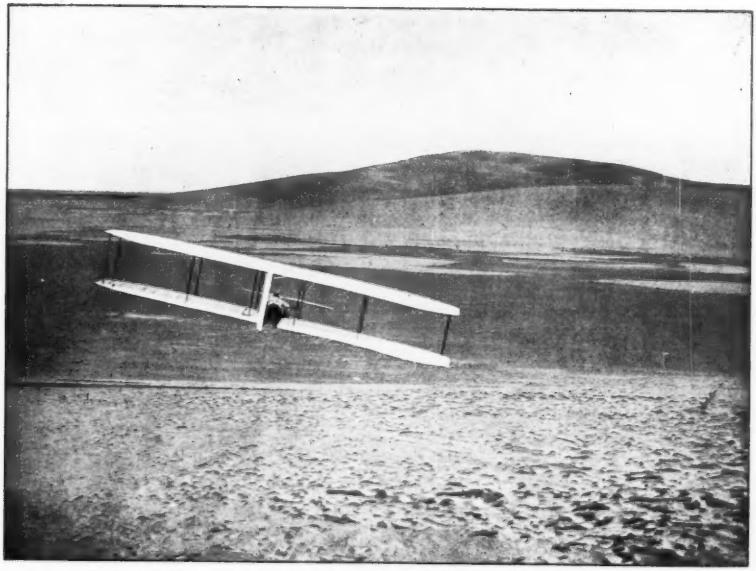


FIG. 7. "Warping" on a turn.

FIG. 8. 1903 Camp. Wilbur Wright standing in front of building.

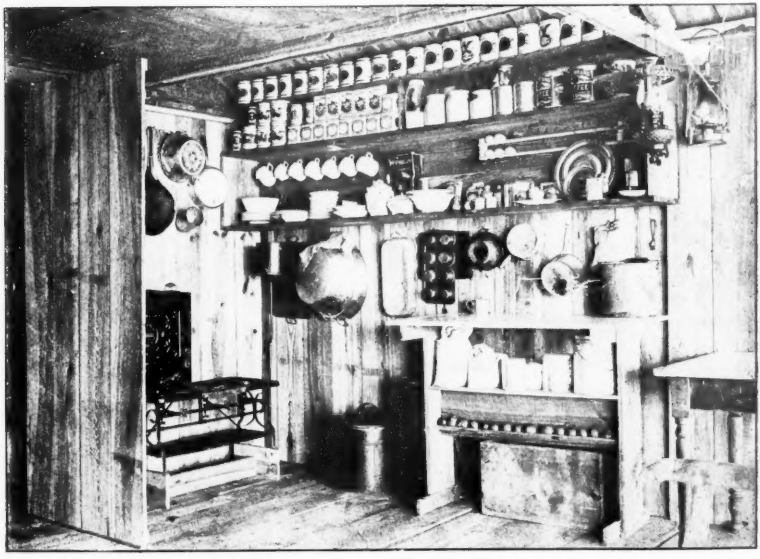


FIG. 9. Interior of kitchen of 1903 Camp.

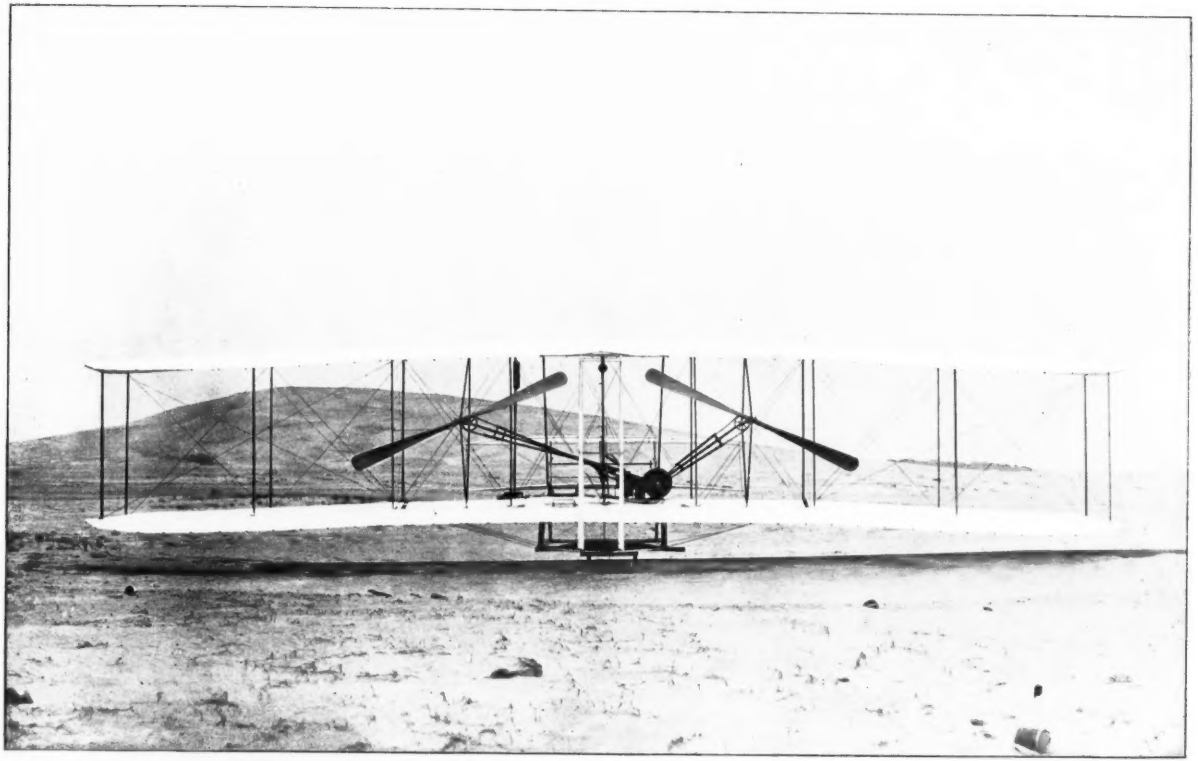


FIG. 10. First Flyer, 1903.

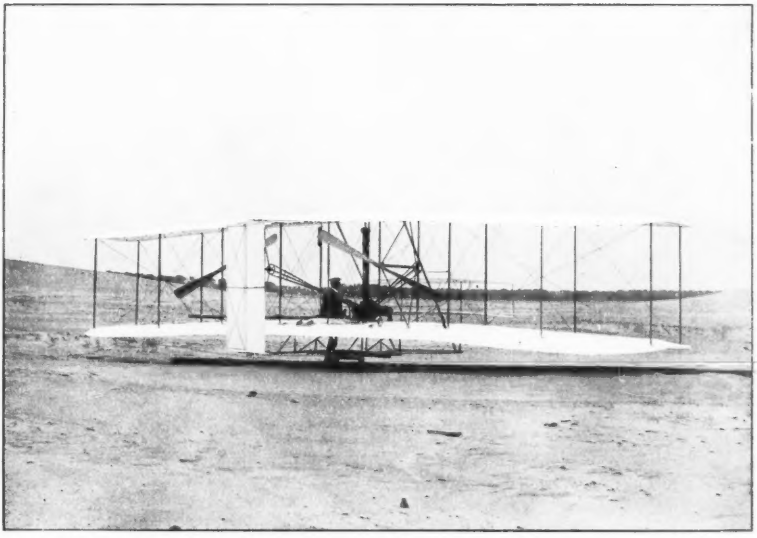


FIG. 11. Another view of the 1903 Flyer.

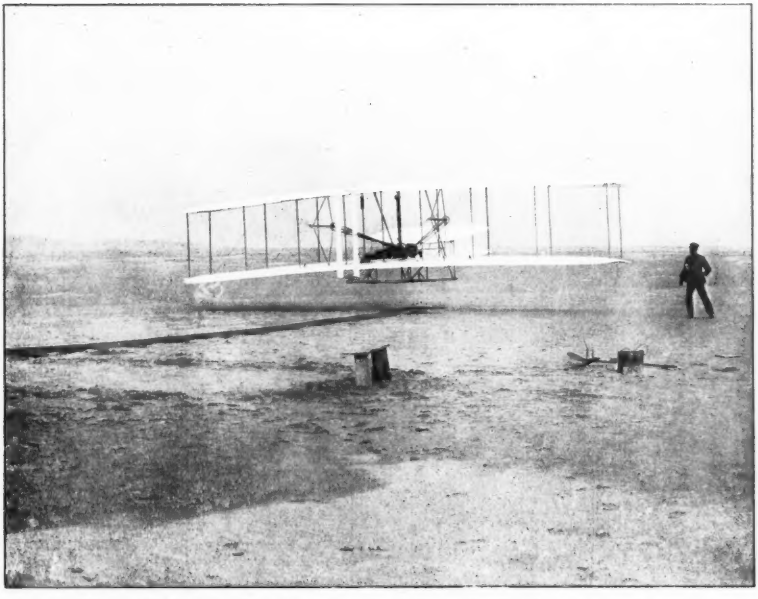
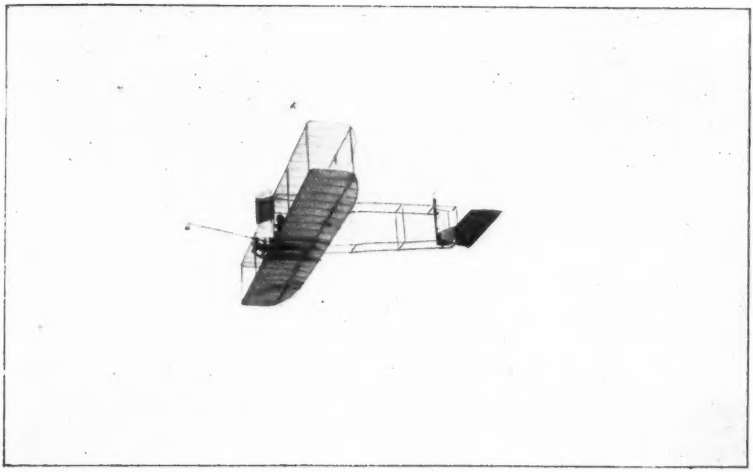
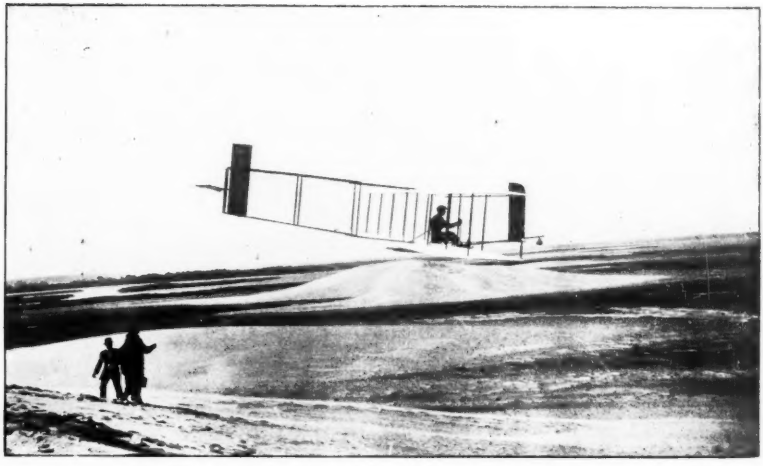


FIG. 12. First flight of first motor machine, December 17th, 1903, near Kill Devil Hill, Kitty Hawk, N.C., against a wind of 24 miles an hour. This picture represents the machine just after starting from the rail established for acquiring initial velocity, but no thrust was given outside of its own power.



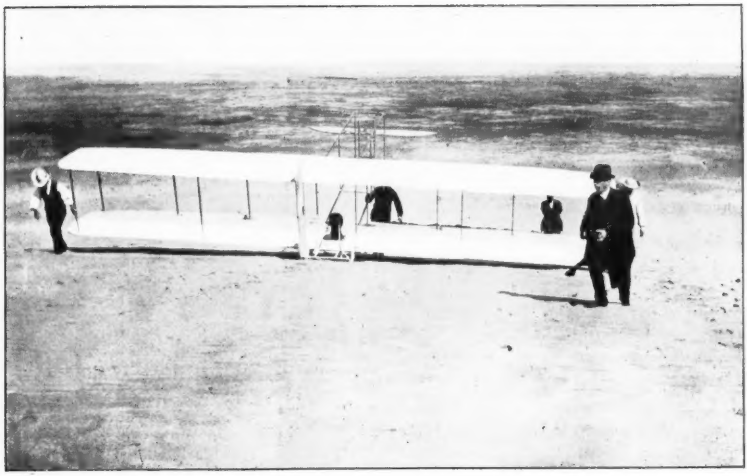
(*Photo, Ness Harwood.*)

FIG. 13. Continuation of gliding experiments in 1911 by Orville Wright and Alec Ogilvie. Ogilvie up.



(*Ness Harwood.*)

FIG. 14. Ogilvie landing after a flight.

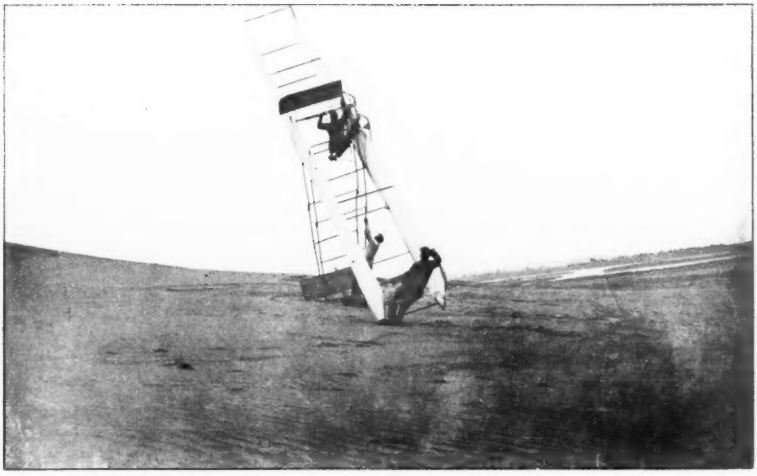


(Ness Harwood.)

FIG. 15. Carrying the glider uphill after a flight.

(Ness Harwood.)

FIG. 16. Orville Wright beating the world's record for soaring flight. East of Kill Devil Hill. Time, 10 mins. 1 sec.



(*Ness Harwood.*)

FIG. 17. Orville Wright jumping after glider had turned turtle.

supported me with this object in view. Lord Northcliffe, who knew the history of this action, on hearing of the terms of settlement, wrote to me saying: "I think that Wright has behaved more than generously—nobly, in fact," and I think the British aeroplane industry, now they know what was done for them, will thoroughly endorse Lord Northcliffe's view of the Wrights' generosity towards this country.

Before closing this lecture let me ask you not to attempt to allot any particular portion of the great discovery to one or other of the Brothers. Neither Wilbur nor Orville attempted to divide what they knew to be indivisible, they shared equally in all things. They even planned the house at Dayton together, each with his own rooms, and with no thought of change in their brotherly union. It would therefore be ungenerous to Wilbur, whose memory we honour, to attempt to divide the work which had proved beyond the power of any one brain to accomplish. It required two brains working in unison, two lives, one to be risked while the other watched, two purposes placed before all thoughts of gain or pleasure, to accomplish that which those brothers did for posterity. We therefore honour Wilbur knowing that he will hand half that honour to "Brother Orv."

LORD MONTAGU OF BEAULIEU: Ladies and Gentlemen,—I am sure, after the most interesting lecture we have just heard from Mr. Griffith Brewer—than whom no one could be more qualified to give it—that you will heartily support the vote of thanks to him which I have great pleasure in moving.

He has told us many interesting things. He has brought out very strongly the fact that the Wright Brothers, although they were both busy men, were so persevering and diligent, and it was due to this that they were able to make their work so successful.

I now formally move a vote of thanks to Mr. Griffith Brewer, and will ask Lord Northcliffe if he will be kind enough to second it.

LORD NORTHCLIFFE: My Lord, Ladies and Gentlemen,—I take particular pleasure in coming here to-day, and in seconding the vote of thanks to Mr. Griffith Brewer, because I had the great honour of knowing not only the two Wright boys, but also their sister Katherine.

Mr. Brewer has been through all the great struggle of the Wright patents, and I don't think people have realised how much this has meant.

Mr. Griffith Brewer knew the brothers much more intimately than I did. I merely made their acquaintance, firstly at Le Mans, and afterwards at Pau and other places. I daresay many will remember that when they arrived in Europe, there were very few who believed they were able to fly at all, and I am sure Mr. Griffith Brewer will remember that great day when Wilbur, after all the great care he took in preparing his machine—after, I think, a fortnight of preparation—rose in the air to the immense astonishment of the French people who had been waiting for some days, flew up in the air and disappeared! We are so accustomed to seeing flying machines nowadays, that it is hard to imagine the excitement of seeing a man fly for the first time.

I remember that first aeroplane. It was very rough and very simple. The motor was not only one of very small power, but from the point of view of modern flying machines, very crude. The Wright Brothers, however, were not people who took very much interest in appearances. They knew that their machine could

fly; they took no risks; every part of that machine was perfectly made from a practical point of view, but like many practical things one sees in the United States, there was no attempt at the "finish" which one sees in machines of to-day. The fact remains, however, that after more than a hundred years of experiment with aeroplanes, these two brothers were the first people in the world who made a machine to fly, and flew it.

I make that remark emphatically, because there is one point to which Mr. Griffith Brewer did not call attention, and that is the attempt that has been made to rob the Wright Brothers of the credit of their invention. We have not heard much of that in England, but "a prophet is not without honour save in his own country," and in the United States there have been long and persistent attempts to belittle the work of Wilbur and Orville Wright. I have closely read and followed the history of the hundred years of aeroplane experiments, and I am convinced that the credit of the first flying aeroplane is due to the Wright Brothers, and from the point of practical flying to nobody else. As an Englishman I am in an independent position, and I know that these words of mine will go across the Atlantic, and I believe they will assist in stopping the spread of the insidious suggestion that the Wrights did not invent the aeroplane.

One knows how public delusions grow up, and it requires such a lecture as Mr. Griffith Brewer's, with those unanswerable photographs, and his deep knowledge of the early days of the Wrights, to demonstrate and redemonstrate to the world that Wilbur and Orville did this thing.

I speak of them by their first names, because everyone who knew them got to regard them with great affection. I never knew a more simple, unaffected trio than Wilbur, Orville and Katherine. After they had been in Europe a few weeks, they became world heroes, and when they went to Pau their demonstrations were visited by thousands of people from all parts of Europe—by kings and lesser men, but I don't think that the excitement and interest produced by their extraordinary feat had any effect on that simple American trio at all.

I had from Katherine Wright a fortnight ago, just the same simple kind of letter which one friend writes to another, and the only remark she made about the great work they had done, was to call my attention to two or three of these American attempts to deprive the brothers of the credit which belongs to them on account of their efforts.

They lived in Europe in the most simple way. Wilbur as a rule slept in a little shed by his aeroplane, with a kitchen very much like that one Mr. Griffith Brewer has showed. Their heads were not the least turned by the various attentions bestowed on them, nor by the many great financial offers given them to make demonstrations.

I wrote to Mr. Griffith Brewer a few months ago saying that in accepting £15,000 for inventions invaluable to this country, not only as a source of industry and a means of living for scores of thousands of our people, but as probably the greatest means of defence of this island in the future, the Wrights acted nobly. I think everyone will agree with me that these two brothers did behave in a splendid and handsome manner to Great Britain. (Applause.)

They were Americans of English descent, and had a great affection for this country, and their sister, in writing to me the other day, said, "We here make no pretence at being neutral—we are heart and soul for England and her Allies in this great struggle," and I think that family proved it by taking such a beggarly sum for one of the greatest inventions of the world.

In conclusion, I beg to thank Mr. Griffith Brewer very much for his most interesting lecture. (Loud applause.)

LORD MONTAGU: I think I need hardly put that to the vote, but on your behalf thank Mr. Brewer very heartily for his lecture to-night. (Loud applause.)

Mr. BREWER: I should like to thank everyone for their kindness, and especially Lord Northcliffe for his excellent speech. Thank you all very much indeed.

General RUCK: My Lord, Ladies and Gentlemen,—In rising to propose a vote of thanks to Lord Montagu for his kindness in presiding, I should also like to add a few words of thanks to Mr. Brewer for his very interesting lecture, which will undoubtedly become a regular classic on the Wrights.

I think this is the first time our Society have had the pleasure of congratulating Lord Montagu on his marvellous escape from the Mediterranean, and I am sure you will regard that adventure as affording an example of the pluck and vigour with which he pursues the campaign he is conducting.

We, as a Society, have always believed in the great future of flight, not only from a naval and military point of view, but also in its commercial aspect. I will not venture to enlarge very much on that subject now, because you have all heard what Lord Northcliffe has been saying about it.

I should like to thank Lord Northcliffe on behalf of the Society for the great interest he has always taken in its doings. We have always recognised that he is one of the first and principal supporters of flight.

Lord Montagu also may be considered one of the pioneers, because he has foreshadowed in the most accurate way some of the events now happening in regard to flight in the present war. This shows that he possesses not only the necessary vigour and determination, but also the necessary imagination for tackling this extremely difficult subject—in which we wish him every success—of how best to organise our Flying Services for effectually attacking the enemy. (Cheers.)

General STONE: I have great pleasure in seconding the vote of thanks to Lord Montagu. In taking the chair on this occasion, he has added one more debt to the many for which I hope one day the country will repay him, when it realises all he has done in the interests of flying. (Loud applause.)

General RUCK: I rise to put the vote of thanks to Lord Montagu, but I think there is hardly any necessity to put this formally before you—it is already carried by acclamation. (Loud cheers.)

LORD MONTAGU: It gives me great pleasure to be present on this occasion, and I am so glad to see here my friend Lord Northcliffe.

I went to Pau to see Wright fly in 1908, and I saw then—as anyone must with a spark of imagination—the enormous possibilities of flight. At such times as that, one finds one's mind at once beginning to dwell on the possibilities of the future. No one can tell to-day how far it will go.

It only remains for me to say that when Lord Northcliffe wished me well in my present campaign, which I hope to carry out in an earnest and proper manner, and to wake up this country, he gave me great encouragement; and my desire is, that both the Services and the Government should realise that this is a matter which, if we do not spend on it time and trouble, energy and forethought, might

end in this country becoming an island no longer and to our invasion by the air instead of by the sea. (Applause.)

The meeting terminated at 4.25 p.m.

Extract from "Snap-Shots," 17th November, 1894, of an article by Wilbur Wright.

APPENDIX A.

THE CLUB DISCUSSES LAW.

The libel suits recently instituted by the members of the B.C.A. against Professor Robert were the subject of quite an extended discussion at the last meeting of the Club. "Snap-Shots" representative was a little late in arriving, and accordingly found the Club up to its elbows in the question of whether the words imputed to Professor Robert—"The members of the Board of City Affairs, except one, are thieves"—were in fact libellous and whether the libelled individuals would be able to get damages.

When he arrived the floor was held by a member whose busy moments in his earlier days were devoted to the pressing duties of the Justice of the Peace. He was explaining to the Club a few of the main features of the law of libel, in order that the members might discuss the matter intelligently. He said that before damages could be secured it would be necessary to prove: (1) That the libellous words were actually given public utterance by the defendant; (2) that they were unquestionably applied to the plaintiff; (3) that the words complained of were really libellous in their nature.

The law having been laid down, the mathematical member arose to inquire why all four members of the B.C.A. had brought suits. He thought it was admitted by everyone that Professor Robert had libelled no more than three at the utmost. Yet here are four suits. He was inclined to sympathise with the Professor on this account, because he was evidently being falsely accused. He himself had not been able to prove the truth or falsity of the Professor's charge that three members of the B.C.A. were thieves, because he had not had an opportunity to add up the figures in their private bank books, but when all four jump on to Mr. Robert for $20,000 each, although one had been expressly excepted from the accusation, it was evident to any unprejudiced person that an attempt was being made to gouge Mr. Robert out of $20,000. For his part, he questioned whether those who would thus attempt to cheat a fellow-citizen out of so great a sum were deserving of any better name than "thieves." He did not wish to pass judgment hastily in a matter on which he was not fully informed, but he could not help but admit that the B.C.A. were dealing dishonestly with Mr. Robert in bringing four suits, aggregating $80,000, instead of three suits, aggregating only $60,000.

Another member, well known from contributions to the Dayton papers over the signature of "Reform," said he was above all questions of mere money matters, but he wished to inquire which one of the four members of the B.C.A. was supposed to be the honest man. From the unanimity with which the accusation had been resented it would appear that all four had found the shoe a more or less close fit. He hoped the suits would be pushed to a speedy termination, in order that the honest member might be found, if possible.

The Higher Critic said he had been greatly surprised at the lack of business acumen displayed by Professor Robert in the matter. The manner in which he had claimed everything in sight in the river bed had led him to suppose that Mr. Robert would always get all he could for the money. But here, if reports are to

be trusted, he had contented himself with calling only three of the B.C.A. men thieves, when it would have been just as cheap to apply the name to all four. This moderation was so entirely foreign to his character, as displayed in all the matters connected with the river bed fill, that it was his opinion that Mr. Robert had really included all four in his statement, and that the words "except one" were an interpolation, probably introduced by someone for the purpose of flattering the member of the B.C.A. to whom the matter was first reported. The fact that after investigation all four members brought suit further tended to confirm the correctness of this hypothesis.

Another member said he had wondered much in what order the suits would be taken up, as it was evident that if justice were done the fourth man would not get a cent, while his three fellows would be gloating over $20,000 each. He had his doubts whether there was any member of the B.C.A. who would quietly allow $20,000 to slip through his fingers. He suggested that they should either draw cuts for first turn in court or else lump the damages and take fifteen thousand each.

Another member thought that the B.C.A. members had brought the suits just as a "bluff," and that each was more anxious to have the public believe himself the one excepted by Mr. Robert, than to revel in the spoils of Mr. Robert's money bags. It was his opinion that when the cases came up for trial each member of the B.C.A. would have such a strong array of witnesses to his snow-white character, and be represented by such brilliant counsel, that the jury in each case would find the plaintiff the one whose lamb-like purity and honesty had so excited the admiration of Mr. Robert, that, even in the heat of indignation, he paused to except him from the general accusation of thievery; and that, as a result, Mr. Robert would enjoy the luxury of calling 75 per cent. of the B.C.A. thieves without paying a cent. This speaker further said that he rejected as unnatural the hypothesis advanced by a previous speaker that Mr. Robert had really called all four members thieves. If he had really applied this opprobrious epithet to all four he would have driven them to desperation, and left no escape either for them or for himself. But by excepting one he left an avenue of which both he and they would, no doubt, avail themselves.

Another member of the Club could see in the whole proceeding only another illustration of the evil of monopolies. The attempt of each member to monopolise the honour of being the only reputed honest member of the Board was likely to result in another failure of justice. For his part he wished to be put on record as a friend of the labouring man.

The poet now arose, and, unrolling a great package of manuscript, began to read a poem after the style of the Iliad and the Aenead, in which the heroic actions of the parties to the legal fight are set forth in blank verse. We confess with shame that before one-half had been read our nodding head was given over to sleep, but the energy and spirit with which he recited the closing song which the B.C.A. quartette are supposed to sing in honour of the closing of the affair aroused us in time to catch the final verse, which we gladly reproduce:—

"Yes, four are we of the B.C.A.,
And for calling us thieves he'd surely pay,
But he left one out,
So the jury, in doubt,
Found all of us honest, they say."

APPENDIX B.

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CXXXII.

SOME AERONAUTICAL EXPERIMENTS.

MR. WILBUR WRIGHT, Dayton, O.

Presented September 18, 1901.

INTRODUCTION BY PRESIDENT CHANUTE.

Engineers have, until recent years, fought shy of anything relating to aerial navigation. Those who ventured, in spite of the odium attached to that study, to look into it at all, became very soon satisfied that the great obstacle in the way was the lack of a motor sufficiently light to sustain its weight and that of an aeroplane, upon the air. Fifteen years ago the lightest steam motor was the marine engine weighing 60 pounds to the H.P., while the gas engine weighed very much more; the locomotive weighed 200 pounds per H.P. During the past fifteen years a great change has taken place. Steam motors have been produced weighing only 10 pounds per horse-power, and gas engines have been lightened down to 12½ to 15 pounds per horse-power, so that the status, so far as engineers are concerned, is very greatly changed, and there is some hope that, for some limited purposes at least, man will eventually be able to fly through the air. There is, however, before that can be carried out—before a motor can be applied to a flying machine—an important problem to solve—that of safety or that of stability.

I had the honour of telling you, some four or five years ago, something about the progress that had been made up to that time. Since then further advances have been made by two gentlemen from Dayton, Ohio—Mr. Wilbur Wright and Mr. Orville Wright, who tried some very interesting experiments in October, 1900, about a year ago. These experiments were conducted on the sea shore of North Carolina, and were again resumed last July. These gentlemen have been bold enough to attempt some things which neither Lilienthal nor Pilcher nor myself dared to do. They have used surfaces very much greater in extent than those which hitherto had been deemed safe, and they have accomplished very remarkable results, part of which it was my privilege to see on a visit which I paid to their camp about a month ago.

I thought it would be interesting to the members of this society to be the first to learn of the results accomplished, and therefore, I have the honour of presenting to you Mr. Wilbur Wright.

The difficulties which obstruct the pathway to success in flying machine construction are of three general classes: (1) Those which relate to the construction of the sustaining wings. (2) Those which relate to the generation and application of the power required to drive the machine through the air. (3) Those relating to the balancing and steering of the machine after it is actually in flight. Of these difficulties two are already to a certain extent solved. Men already know how to construct wings or aeroplanes which, when driven through the air at sufficient speed, will not only sustain the weight of the wings themselves, but also that of the engine, and of the engineer as well. Men also know how to build engines and screws of sufficient lightness and power to drive these planes at sustaining speed. As long ago as 1893 a machine weighing 8,000lbs. demonstrated its power both to lift itself from the ground and to maintain a speed of from 30 to 40 miles per hour; but it came to grief in an accidental free flight, owing to the inability of the operators to balance and steer it properly. This inability to balance and

steer still confronts students of the flying problem, although nearly ten years have passed. When this one feature has been worked out the age of flying machines will have arrived, for all other difficulties are of minor importance.

The person who merely watches the flight of a bird gathers the impression that the bird has nothing to think of but the flapping of its wings. As a matter of fact, this is a very small part of its mental labour. To even mention all the things the bird must constantly keep in mind in order to fly securely through the air would take a considerable part of the evening. If I take this piece of paper, and after placing it parallel with the ground, quickly let it fall, it will not settle steadily down as a staid, sensible piece of paper ought to do, but it insists on contravening every recognised rule of decorum, turning over and darting hither and thither in the most erratic manner, much after the style of an untrained horse. Yet this is the style of steed that men must learn to manage before flying can become an everyday sport. The bird has learned this art of equilibrium, and learned it so thoroughly that its skill is not apparent to our sight. We only learn to appreciate it when we try to imitate it. Now, there are two ways of learning how to ride a fractious horse: one is to get on him and learn by actual practice how each motion and trick may be best met; the other is to sit on a fence and watch the beast awhile, and then retire to the house and at leisure figure out the best way of overcoming his jumps and kicks. The latter system is the safest; but the former, on the whole, turns out the larger proportion of good riders. It is very much the same in learning to ride a flying machine; if you are looking for perfect safety you will do well to sit on a fence and watch the birds; but if you really wish to learn you must mount a machine and become acquainted with its tricks by actual trial.

Herr Otto Lilienthal seems to have been the first man who really comprehended that balancing was the *first* instead of the *last* of the great problems in connection with human flight. He began where others left off, and thus saved the many thousands of dollars that it had theretofore been customary to spend in building and fitting expensive engines to machines which were uncontrollable when tried. He built a pair of wings of a size suitable to sustain his own weight, and made use of gravity as his motor. This motor not only cost him nothing to begin with, but it required no expensive fuel while in operation, and never had to be sent to the shop for repairs. It had one serious drawback, however, in that it always insisted on fixing the conditions under which it would work. These were that the man should first betake himself and machine to the top of a hill and fly with a downward as well as a forward motion. Unless these conditions were complied with, gravity served no better than a balky horse—it would not work at all. Although Lilienthal must have thought the conditions were rather hard, he nevertheless accepted them till something better should turn up; and in this manner he made some two thousand flights, in a few cases landing at a point more than a thousand feet distant from his place of starting. Other men, no doubt, long before had thought of trying such a plan. Lilienthal not only thought, but acted; and in so doing probably made the greatest contribution to the solution of the flying problem that has ever been made by any one man. He demonstrated the feasibility of actual practice in the air, without which success is impossible. Herr Lilienthal was followed by Mr. Pilcher, a young English engineer, and by Mr. Chanute, a distinguished member of the society I now address. A few others have built machines, but nearly all that is of real value is due to the experiments conducted under the direction of the three men just mentioned.

The balancing of a gliding or flying machine is very simple in theory. It merely consists in causing the centre of pressure to coincide with the centre of gravity. But in actual practice there seems to be an almost boundless incompatibility of temper which prevents their remaining peaceably together for a single instant, so that the operator, who in this case acts as peacemaker, often suffers injury to himself while attempting to bring them together. If a wind strikes a

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vertical plane, the pressure on that part to one side of the centre will exactly balance that on the other side, and the part above the centre will balance that below. This point we call the centre of pressure. But if the plane be slightly inclined, the pressure on that part nearest the wind is increased, and the pressure on the other part decreased, so that the centre of pressure is now located, not in the centre of the surface, but a little toward the side which is in advance. If the plane be still further inclined the centre of pressure will move still further forward. And if the wind blow a little to one side, it will also move over as if to meet it. Now, since neither the wind nor the machine for even an instant maintains exactly the same direction and velocity, it is evident that the man who would trace the course of the centre of pressure must be very quick of mind; and he who would attempt to move his body to that spot at every change must be very active indeed. Yet this is what Herr Lilienthal attempted to do, and did do with most remarkable skill, as his two thousand glides sufficiently attest. However, he did not escape being overturned by wind gusts several times, and finally lost his life through a breakage of his machine, due to defective construction. The Pilcher machine was similar to that of Lilienthal, and, like it, seems to have been structurally weak; for on one occasion, while exhibiting the flight of his machine to several members of the Aëronautical Society of Great Britain, it suddenly collapsed and fell to the ground, causing injuries to the operator which proved sadly fatal. The method of management of this machine differed in no important respect from that of Lilienthal, the operator shifting his body to make the centres of pressure and gravity coincide. Although the fatalities which befel the designers of these machines were due to the lack of structural strength, rather than to lack of control, nevertheless it had become clear to the students of the problem that a more perfect method of control must be evolved. The Chanute machines marked a great advance in both respects. In the multiple wing machine the tips folded slightly backward under the pressure of wind gusts, so that the travel of the centre of pressure was thus largely counterbalanced. The guiding of the machine was done by a slight movement of the operator's body toward the direction in which it was desired that the machine should go. The double-deck machine built and tried at the same time marked a very great structural advance, as it was the first in which the principles of the modern truss bridges were fully applied to flying machine construction. This machine, in addition to its greatly improved construction and general design of parts, also differed from the machine of Lilienthal in the operation of its tail. In the Lilienthal machine the tail, instead of being fixed in one position, was prevented by a stop from folding downward beyond a certain point, but was free to fold upward without any hindrance. In the Chanute machine the tail was at first rigid, but afterward, at the suggestion of Mr. Herring, it was held in place by a spring that allowed it to move slightly either upward or downward with reference to its normal position, thus modifying the action of the wind gusts upon it, very much to its advantage. The guiding of the machine was effected by slight movements of the operator's body, as in the multiple-wing machines. Both these machines were much more manageable than the Lilienthal type, and their structural strength, notwithstanding their extreme lightness, was such that no fatalities, or even accidents, marked the glides made with them, although winds were successfully encountered much greater in violence than any which previous experimenters had dared to attempt.

My own active interest in aeronautical problems dates back to the death of Lilienthal in 1896. The brief notice of his death which appeared in the telegraphic news at that time aroused a passive interest which had existed from my childhood, and led me to take down from the shelves of our home library a book on "Animal Mechanism," by Prof. Marey, which I had already read several times. From this I was led to read more modern works, and as my brother soon became equally interested with myself, we soon passed from the reading to the thinking, and finally to the working stage. It seemed to us that the main reason why the problem had remained so long unsolved was that no one had been able

to obtain any adequate practice. We figured that Lilienthal in five years of time had spent only about five hours in actual gliding through the air. The wonder was not that he had done so little, but that he had accomplished so much. It would not be considered at all safe for a bicycle rider to attempt to ride through a crowded city street after only five hours' practice, spread out in bits of ten seconds each over a period of five years; yet Lilienthal with this brief practice was remarkably successful in meeting the fluctuations and eddies of wind gusts. We thought that if some method could be found by which it would be possible to practice by the hour instead of by the second there would be hope of advancing the solution of a very difficult problem. It seemed feasible to do this by building a machine which would be sustained at a speed of 18 miles per hour, and then finding a locality where winds of this velocity were common. With these conditions a rope attached to the machine to keep it from floating backward would answer very nearly the same purpose as a propeller driven by a motor, and it would be possible to practice by the hour, and without any serious danger, as it would not be necessary to rise far from the ground, and the machine would not have any forward motion at all. We found, according to the accepted tables of air pressures on curved surfaces, that a machine spreading 200 square feet of wing surface would be sufficient for our purpose, and that places could easily be found along the Atlantic coast where winds of 16 to 25 miles were not at all uncommon. When the winds were low it was our plan to glide from the tops of sand hills, and when they were sufficiently strong to use a rope for our motor and fly over one spot. Our next work was to draw up the plans for a suitable machine. After much study we finally concluded that tails were a source of trouble rather than of assistance, and therefore we decided to dispense with them altogether. It seemed reasonable that if the body of the operator could be placed in a horizontal position instead of the upright, as in the machines of Lilienthal, Pilcher and Chanute, the wind resistance could be very materially reduced, since only one square foot instead of five would be exposed. As a full half-horse-power could be saved by this change, we arranged to try at least the horizontal position. Then the method of control used by Lilienthal, which consisted in shifting the body, did not seem quite as quick or effective as the case required; so, after long study, we contrived a system consisting of two large surfaces on the Chanute double-deck plan, and a smaller surface placed a short distance in front of the main surfaces in such a position that the action of the wind upon it would counterbalance the effect of the travel of the centre of pressure on the main surfaces. Thus changes in the direction and velocity of the wind would have little disturbing effect, and the operator would be required to attend only to the steering of the machine, which was to be effected by curving the forward surface up or down. The lateral equilibrium and the steering to right or left was to be attained by a peculiar torsion of the main surfaces, which was equivalent to presenting one end of the wings at a greater angle than the other. In the main frame a few changes were also made in the details of construction and trussing employed by Mr. Chanute. The most important of these were: (1) The moving of the forward main cross-piece of the frame to the extreme front edge; (2) the encasing in the cloth of all cross-pieces and ribs of the surfaces; (3) a rearrangement of the wires used in trussing the two surfaces together, which rendered it possible to tighten all the wires by simply shortening two of them.

With these plans we proceeded in the summer of 1900 to Kitty Hawk, North Carolina, a little settlement located on the strip of land that separates Albemarle Sound from the Atlantic Ocean. Owing to the impossibility of obtaining suitable material for a 200-square-foot machine, we were compelled to make it only 165 square feet in area, which, according to the Lilienthal tables, would be supported at an angle of three degrees in a wind of about 21 miles per hour. On the very day that the machine was completed the wind blew from 25 to 30 miles per hour, and we took it out for trial as a kite. We found that while it was supported with a man on it in a wind of about 25 miles, its angle was much nearer 20 degrees

than three degrees. Even in gusts of 30 miles the angle of incidence did not get as low as three degrees, although the wind at this speed has more than twice the lifting power of a 21-mile wind. As winds of 30 miles per hour are not plentiful on clear days, it was at once evident that our plan of practising by the hour, day after day, would have to be postponed. Our system of twisting the surfaces to regulate the lateral balance was tried and found to be much more effective than shifting the operator's body. On subsequent days, when the wind was too light to support the machine with a man on it, we tested it as a kite, working the rudders by cords reaching to the ground. The results were very satisfactory, yet we were well aware that this method of testing is never wholly convincing until the results are confirmed by actual gliding experience.

We then turned our attention to making a series of actual measurements of the lift and drift of the machine under various loads. So far as we were aware, this had never previously been done with any full-size machine. The results obtained were most astonishing, for it appeared that the total horizontal pull of the machine, while sustaining a weight of 52lbs., was only 8.5lbs., which was less than had previously been estimated for head resistance of the framing alone. Making allowance for the weight carried, it appeared that the head resistance of the framing was but little more than 50 per cent. of the amount which Mr. Chanute had estimated as the head resistance of the framing of his machine. On the other hand, it appeared sadly deficient in lifting power as compared with the calculated lift of curved surfaces of its size. This deficiency we supposed might be due to one or more of the following causes:—(1) That the depth of the curvature of our surfaces was insufficient, being only about one in 22, instead of one in 12. (2) That the cloth used in our wings was not sufficiently air-tight. (3) That the Lilienthal tables might themselves be somewhat in error. We decided to arrange our machine for the following year so that the depth of curvature of its surfaces could be varied at will and its covering air-proofed.

Our attention was next turned to gliding, but no hill suitable for the purpose could be found near our camp at Kitty Hawk. This compelled us to take the machine to a point four miles south, where the Kill Devil sand hill rises from the flat sand to a height of more than 100 feet. Its main slope is toward the north-east, and has an inclination of 10 degrees. On the day of our arrival the wind blew about 25 miles an hour, and as we had had no experience at all in gliding, we deemed it unsafe to attempt to leave the ground. But on the day following, the wind having subsided to 14 miles per hour, we made about a dozen glides. It had been the original intention that the operator should run with the machine to obtain initial velocity, and assume the horizontal position only after the machine was in free flight. When it came time to land he was to resume the upright position and alight on his feet, after the style of previous gliding experimenters. But on actual trial we found it much better to employ the help of two assistants in starting, which the peculiar form of our machine enabled us readily to do; and in landing we found that it was entirely practicable to land while still reclining in a horizontal position upon the machine. Although the landings were made while moving at speeds of more than 20 miles an hour, neither machine nor operator suffered any injury. The slope of the hill was 9.5 deg., or a drop of one foot in six. We found that after attaining a speed of about 25 to 30 miles with reference to the wind, or 10 to 15 miles over the ground, the machine not only glided parallel to the slope of the hill, but greatly increased its speed, thus indicating its ability to glide on a somewhat less angle than 9.5 deg., when we should feel it safe to rise higher from the surface. The control of the machine proved even better than we had dared to expect, responding quickly to the slightest motion of the rudder. With these glides our experiments for the year 1900 closed. Although the hours and hours of practice we had hoped to obtain finally dwindled down to about two minutes, we were very much pleased with the general results of the trip, for, setting out as we did with almost revolutionary theories on many points and an entirely untried form of machine, we considered it quite a point to

be able to return without having our pet theories completely knocked on the head by the hard logic of experience, and our own brains dashed out in the bargain. Everything seemed to us to confirm the correctness of our original opinions—(1) that practice is the key to the secret of flying; (2) that it is practicable to assume the horizontal position; (3) that a smaller surface set at a negative angle in front of the main bearing surfaces, or wings, will largely counteract the effect of the fore and aft travel of the centre of pressure; (4) that steering up and down can be attained with a rudder without moving the position of the operator's body; (5) that twisting the wings so as to present their ends to the wind at different angles is a more prompt and efficient way of maintaining lateral equilibrium than shifting the body of the operator.

When the time came to design our new machine for 1901 we decided to make it exactly like the previous machine in theory and method of operation. But as the former machine was not able to support the weight of the operator when flown as a kite, except in very high winds and at very large angles of incidence, we decided to increase its lifting power. Accordingly, the curvature of the surfaces was increased to one in 12, to conform to the shape on which Lilienthal's table was based, and to be on the safe side we decided also to increase the area of the machine from 165 square feet to 308 square feet, although so large a machine had never before been deemed controllable. The Lilienthal machine had an area of 151 square feet; that of Pilcher, 165 square feet; and the Chanute double-decker, 134 square feet. As our system of control consisted in a manipulation of the surfaces themselves instead of shifting the operator's body, we hoped that the new machine would be controllable, notwithstanding its great size. According to calculations, it would obtain support in a wind of 17 miles per hour with an angle of incidence of only three degrees.

Our experience of the previous year having shown the necessity of a suitable building for housing the machine, we erected a cheap frame building, 16 feet wide, 25 feet long, and 7 feet high at the eaves. As our machine was 22 feet wide, 14 feet long (including the rudder), and about 6 feet high, it was not necessary to take the machine apart in any way in order to house it. Both ends of the building, except the gable parts, were made into doors which hinged above, so that when opened they formed an awning at each end and left an entrance the full width of the building. We went into camp about the middle of July, and were soon joined by Mr. E. C. Huffaker, of Tennessee, an experienced aeronautical investigator in the employ of Mr. Chanute, by whom his services were kindly loaned, and by Dr. G. A. Spratt, of Pennsylvania, a young man who has made some valuable investigations of the properties of variously curved surfaces and the travel of the centre of pressure thereon. Early in August Mr. Chanute came down from Chicago to witness our experiments, and spent a week in camp with us. These gentlemen, with my brother and myself, formed our camping party, but in addition we had in many of our experiments the valuable assistance of Mr. W. J. Tate and Mr. Dan Tate, of Kitty Hawk.

The machine was completed and tried for the first time on July 27th in a wind blowing about 13 miles an hour. The operator having taken a position where the centre of pressure was supposed to be, an attempt at gliding was made; but the machine turned downward and landed after going only a few yards. This indicated that the centre of gravity was too far in front of the centre of pressure. In the second attempt the operator took a position several inches further back, but the result was much the same. He kept moving further and further back with each trial, till finally he occupied a position nearly a foot back of that at which we had expected to find the centre of pressure. The machine then sailed off and made an undulating flight of a little more than 300 feet. To the onlookers this flight seemed very successful, but to the operator it was known that the full power of the rudder had been required to keep the machine from either running into the ground or rising so high as to lose all headway. In the 1900 machine

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one-fourth as much rudder action had been sufficient to give much better control. It was apparent that something was radically wrong, though we were for some time unable to locate the trouble. In one glide the machine rose higher and higher till it lost all headway. This was the position from which Lilienthal had always found difficulty to extricate himself, as his machine then, in spite of his greatest exertions, manifested a tendency to dive downward almost vertically and strike the ground head on with frightful velocity. In this case a warning cry from the ground caused the operator to turn the rudder to its full extent and also to move his body slightly forward. The machine then settled slowly to the ground, maintaining its horizontal position almost perfectly, and landed without any injury at all. This was very encouraging, as it showed that one of the very greatest dangers in machines with horizontal tails had been overcome by the use of a front rudder. Several glides later the same experience was repeated with the same result. In the latter case the machine had even commenced to move backward, but was nevertheless brought safely to the ground in a horizontal position. On the whole, this day's experiments were encouraging, for while the action of the rudder did not seem at all like that of our 1900 machine, yet we had escaped without difficulty from positions which had proved very dangerous to preceding experimenters, and after less than one minute's actual practice had made a glide of more than 300 feet, at an angle of descent of 10 degrees, and with a machine nearly twice as large as had previously been considered safe. The trouble with its control, which has been mentioned, we believed could be corrected when we should have located its cause. Several possible explanations occurred to us, but we finally concluded that the trouble was due to a reversal of the direction of the travel of the centre of pressure at small angles. In deeply curved surfaces the centre of pressure at 90 degrees is near the centre of the surface, but moves forward as the angle becomes less, till a certain point is reached, varying with the depth of curvature. After this point is passed the centre of pressure, instead of continuing to move forward, with the decreasing angle, turns and moves rapidly toward the rear. The phenomena are due to the fact that at small angles the wind strikes the forward part of the surface on the *upper* side instead of the lower, and thus this part altogether ceases to lift, instead of being the most effective part of all, as is the case of the plane. Lilienthal had called attention to the danger of using surfaces with a curvature as great as one in eight, on account of this action on the upper side; but he seems never to have investigated the curvature and angle at which the phenomena entirely ceases. My brother and I had never made any original investigation of the matter, but assumed that a curvature of one in 12 would be safe, as this was the curvature on which Lilienthal based his tables. However, to be on the safe side, instead of using the arc of a circle, we had made the curve of our machine very abrupt at the front, so as to expose the least possible area to this downward pressure. While the machine was building Messrs. Huffaker and Spratt had suggested that we would find this reversal of the centre of pressure, but we believed it sufficiently guarded against. Accordingly, we were not at first disposed to believe that this reversal actually existed in our machine, although it offered a perfect explanation of the action we had noticed in gliding. Our peculiar plan of control by forward surfaces, instead of tails, was based on the assumption that the centre of pressure would continue to move farther and farther forward, as the angle of incidence became less, and it will be readily perceived that it would make quite a difference if the front surface, instead of counteracting this assumed forward travel, should in reality be expediting an actual backward movement. For several days we were in a state of indecision, but were finally convinced by observing the following phenomena:—(Fig. 1). We had removed the upper surface from the machine and were flying it in a wind to see at what angles it would be supported in winds of different strengths. We noticed that in light winds it flew in the upper position shown in the figure, with a strong upward pull on the cord *c*. As the wind became stronger the angle of incidence became less, and the surface flew in the position shown in the middle of

the figure, with a slight horizontal pull. But when the wind became still stronger it took the lower position shown in the figure, with a strong downward pull. It at once occurred to me that here was the answer to our problem, for it is evident that in the first case the centre of pressure was in front of the centre of gravity and thus pushed up the front edge; in the second case they were in coincidence, and the surface in equilibrium; while in the third case the centre of pressure had reached a point even behind the centre of gravity, and there was therefore a downward pull on the cord. This point having been definitely settled, we proceeded to truss down the ribs of the whole machine, so as to reduce the depth of curvature. In Fig. 2, line 1 shows the original curvature; line 2, the curvature when supporting the operator's weight; and line 3, the curvature after trussing.

FIG. 1.

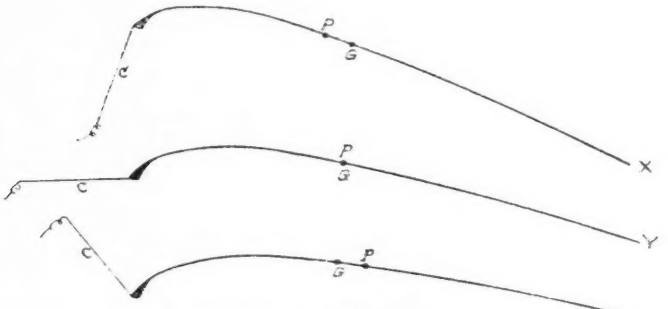
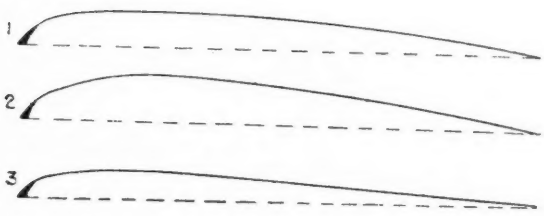


FIG. 2



On resuming our gliding we found that the old conditions of the preceding year had returned, and after a few trials, made a glide 366 feet and soon after one of 389 feet. The machine, with its new curvature, never failed to respond promptly to even small movements of the rudder. The operator could cause it to almost skim the ground, following the undulations of its surface, or he could cause it to sail out almost on a level with the starting point, and passing high above the foot of the hill, gradually settle down to the ground. The wind on this day was blowing 11 to 14 miles per hour. The next day, the conditions being favourable, the machine was again taken out for trial. This time the velocity of the wind was 18 to 22 miles per hour. At first we felt some doubt as to the safety of attempting free flight in so strong a wind, with a machine of over 300 square feet and a practice of less than five minutes spent in actual flight. But after several

preliminary experiments we decided to try a glide. The control of the machine seemed so good that we then felt no apprehension in sailing boldly forth. And thereafter we made glide after glide, sometimes following the ground closely and sometimes sailing high in the air. Mr. Chanute had his camera with him and took pictures of some of these glides, several of which are among those shown.

We made glides on subsequent days, whenever the conditions were favourable. The highest wind thus experimented in was a little over 12 metres per second—nearly 27 miles per hour.

It had been our intention when building the machine to do the larger part of the experimenting in the following manner:—When the wind blew 17 miles an hour, or more, we would attach a rope to the machine and let it rise as a kite with the operator upon it. When it should reach a proper height the operator would cast off the rope and glide down to the ground just as from the top of a hill. In this way we would be saved the trouble of carrying the machine uphill after each glide, and could make at least 10 glides in the time required for one in the other way. But when we came to try it we found that a wind of 17 miles, as measured by Richards' anemometer, instead of sustaining the machine with its operator, a total weight of 240lbs., at an angle of incidence of three degrees, in reality would not sustain the machine alone—100lbs.—at this angle. Its lifting capacity seemed scarcely one-third of the calculated amount. In order to make sure that this was not due to the porosity of the cloth, we constructed two small experimental surfaces of equal size, one of which was air-proofed and the other left in its natural state; but we could detect no difference in their lifting powers. For a time we were led to suspect that the lift of curved surfaces little exceeded that of planes of the same size, but further investigation and experiment led to the opinion that (1) the anemometer used by us over-recorded the true velocity of the wind by nearly 15 per cent.; (2) that the well-known Smeaton coefficient of .005 V^2 for the wind pressure at 90 degrees is probably too great by at least 20 per cent.; (3) that Lilienthal's estimate that the pressure on a curved surface having an angle of incidence of three degrees equals .545 of the pressure at 90 degrees is too large, being nearly 50 per cent. greater than very recent experiments of our own with a special pressure testing machine indicate; (4) that the superposition of the surfaces somewhat reduced the lift per square foot, as compared with a single surface of equal area.

In gliding experiments, however, the amount of lift is of less relative importance than the ratio of lift to drift, as this alone decides the angle of gliding descent. In a plane the pressure is always perpendicular to the surface, and the ratio of lift to drift is therefore the same as that of the cosine to the sine of the angle of incidence. But in curved surfaces a very remarkable situation is found. The pressure, instead of being uniformly normal to the chord of the arc, is usually inclined considerably in front of the perpendicular. The result is that the lift is greater and the drift less than if the pressure were normal. Lilienthal was the first to discover this exceedingly important fact, which is fully set forth in his book, "Bird Flight the Basis of the Flying Art," but owing to some errors in the methods he used in making measurements, question was raised by other investigators not only as to the accuracy of his figures, but even as to the existence of any tangential force at all. Our experiments confirm the existence of this force, though our measurements differ considerably from those of Lilienthal. While at Kitty Hawk we spent much time in measuring the horizontal pressure on our unloaded machine at various angles of incidence. We found that at 13 degrees the horizontal pressure was about 23lbs. This included not only the drift proper, or horizontal component of the pressure on the side of the surface, but also the head resistance of the framing as well. The weight of the machine at the time of this test was about 108lbs. Now, if the pressure had been normal to the chord of the surface, the drift proper would have been to the lift (108lbs.) as the sine of 13 degrees is to the cosine of 13 degrees, or $\frac{.22 \times 108}{.97} = 24 +$ lbs.; but this

slightly exceeds the total pull of 23lbs. on our scales. Therefore, it is evident that the average pressure on the surface, instead of being normal to the chord, was so far inclined toward the front that all the head resistance of framing and wires used in the construction was more than overcome. In a wind of 14 miles per hour resistance is by no means a negligible factor, so that tangential is evidently a force of considerable value. In a higher wind, which sustained the machine at an angle of 10 degrees, the pull on the scales was 18lbs. With the pressure normal to the chord the drift proper would have been $\frac{\cdot 17 \times 98^*}{\cdot 98} = 17$lbs., so that, although the higher wind velocity must have caused an increase in the head resistance, the tangential force still came within one pound of overcoming it. After our return from Kitty Hawk we began a series of experiments to accurately determine the amount and direction of the pressure produced on curved surfaces when acted upon by winds at the various angles from zero to 90 degrees. These experiments are not yet concluded, but in general they support Lilienthal in the claim that the curves give pressures more favourable in amount and direction than planes; but we find marked differences in the exact values, especially at angles below 10 degrees. We were unable to obtain direct measurements of the horizontal pressures of the machine with the operator on board, but by comparing the distance travelled in gliding with the vertical fall, it was easily calculated that at a speed of 24 miles per hour the total horizontal resistances of our machine, when bearing the operator, amounted to 40lbs., which is equivalent to about $2\frac{1}{3}$ horse-power. It must not be supposed, however, that a motor developing this power would be sufficient to drive a man-bearing machine. The extra weight of the motor would require either a larger machine, higher speed, or a greater angle of incidence in order to support it, and therefore more power. It is probable, however, that an engine of six horse-power, weighing 100lbs., would answer the purpose. Such an engine is entirely practicable. Indeed, working motors of one-half this weight per horse-power (9lbs. per horse-power) have been constructed by several different builders. Increasing the speed of our machine from 24 to 33 miles per hour reduced the total horizontal pressure from 40 to about 35lbs. This was quite an advantage in gliding, as it made it possible to sail about 15 per cent. further with a given drop. However, it would be of little or no advantage in reducing the size of the motor in a power-driven machine, because the lessened thrust would be counter-balanced by the increased speed per minute. Some years ago Prof. Langley called attention to the great economy of thrust which might be obtained by using very high speeds, and from this many were led to suppose that high speed was essential to success in a motor-driven machine. But the economy to which Prof. Langley called attention was in foot pounds per mile of travel, not in foot pounds per minute. It is the foot pounds per minute that fixes the size of the motor. The probability is that the first flying machines will have a relatively low speed, perhaps not much exceeding 20 miles per hour, but the problem of increasing the speed will be much simpler in some respects than that of increasing the speed of a steamboat; for, whereas in the latter case the size of the engine must increase as the cube of the speed, in the flying machine, until extremely high speeds are reached, the capacity of the motor increases in less than simple ratio; and there is even a decrease in the fuel consumption per mile of travel. In other words, to double the speed of a steamship (and the same is true of the balloon type of airship) eight times the engine and boiler capacity would be required, and four times the fuel consumption per mile of travel; while a flying machine would require engines of less than double the size, and there would be an actual decrease in the fuel consumption per mile of travel. But looking at the matter conversely, the great disadvantage of the flying machine is apparent; for

* The travel of the centre of pressure made it necessary to put sand on the front rudder to bring the centres of gravity and pressure into coincidence, consequently the weight of the machine varied from 98lbs. to 108lbs. in the different tests.

in the latter no flight at all is possible unless the proportion of horse-power to flying capacity is very high; but on the other hand a steamship is a mechanical success if its ratio of horse-power to tonnage is insignificant. A flying machine that would fly at a speed of 50 miles an hour with engines of 1,000 horse-power would not be upheld by its wings at all at a speed of less than 25 miles an hour, and nothing less than 500 horse-power could drive it at this speed. But a boat which could make 40 miles per hour with engines of 1,000 horse-power would still move four miles an hour even if the engines were reduced to one horse-power. The problems of land and water travel were solved in the nineteenth century, because it was possible to begin with small achievements and gradually work up to our present success. The flying problem was left over to the twentieth century, because in this case the art must be highly developed before any flight of any considerable duration at all can be obtained.

However, there is another way of flying which requires no artificial motor, and many workers believe that success will first come by this road. I refer to the soaring flight, by which the machine is permanently sustained in the air by the same means that are employed by soaring birds. They spread their wings to the wind, and sail by the hour, with no perceptible exertion beyond that required to balance and steer themselves. What sustains them is not definitely known, though it is almost certain that it is a rising current of air. But whether it be a rising current or something else, it is as well able to support a flying machine as a bird, if man once learns the art of utilising it. In gliding experiments it has long been known that the rate of vertical descent is very much retarded, and the duration of the flight greatly prolonged, if a strong wind blows *up* the face of the hill parallel to its surface. Our machine, when gliding in still air, has a rate of vertical descent of nearly six feet per second, while in a wind blowing 26 miles per hour up a steep hill we made glides in which the rate of descent was less than two feet per second. And during the larger part of this time, while the machine remained exactly in the rising current, *there was no descent at all, but even a slight rise.* If the operator had had sufficient skill to keep himself from passing beyond the rising current he would have been sustained indefinitely at a higher point than that from which he started. The illustration shows one of these very slow glides at a time when the machine was practically at a standstill. The failure to advance more rapidly caused the photographer some trouble in aiming, as you will perceive. In looking at this picture you will readily understand that the excitement of gliding experiments does not entirely cease with the breaking up of camp. In the photographic dark-room at home we pass moments of as thrilling interest as any in the field, when the image begins to appear on the plate and it is yet an open question whether we have a picture of a flying machine or merely a patch of open sky. These slow glides in rising currents probably hold out greater hope of extensive practice than any other method within man's reach, but they have the disadvantage of requiring rather strong winds or very large supporting surfaces. However, when gliding operators have attained greater skill, they can, with comparative safety, maintain themselves in the air for hours at a time in this way, and thus by constant practice so increase their knowledge and skill that they can rise into the higher air and search out the currents which enable the soaring birds to transport themselves to any desired point by first rising in a circle and then sailing off at a descending angle. This illustration shows the machine, alone, flying in a wind of 35 miles per hour on the face of a steep hill, 100 feet high. It will be seen that the machine not only pulls upward, but also pulls forward in the direction from which the wind blows, thus overcoming both gravity and the speed of the wind. We tried the same experiment with a man on it, but found danger that the forward pull would become so strong that the men holding the ropes would be dragged from their insecure foothold on the slope of the hill. So this form of experimenting was discontinued after four or five minutes' trial.

In looking over our experiments of the past two years, with models and full-size machines, the following points stand out with clearness:—

1. That the lifting power of a large machine, held stationary in a wind at a small distance from the earth, is much less than the Lilienthal table and our own laboratory experiments would lead us to expect. When the machine is moved through the air, as in gliding, the discrepancy seems much less marked.

2. That the ratio of drift to lift in well-shaped surfaces is less at angles of incidence of five degrees to 12 degrees than at an angle of three degrees.

3. That in arched surfaces the centre of pressure at 90 degrees is near the centre of the surface, but moves slowly forward as the angle becomes less, till a critical angle varying with the shape and depth of the curve is reached, after which it moves rapidly toward the rear till the angle of no lift is found.

4. That with similar conditions large surfaces may be controlled with not much greater difficulty than small ones, if the control is effected by manipulation of the surfaces themselves, rather than by a movement of the body of the operator.

5. That the head resistances of the framing can be brought to a point much below that usually estimated as necessary.

6. That tails, both vertical and horizontal, may with safety be eliminated in gliding and other flying experiments.

7. That a horizontal position of the operator's body may be assumed without excessive danger, and thus the head resistance reduced to about one-fifth that of the upright position.

8. That a pair of superposed, or tandem, surfaces has less lift in proportion to drift than either surface separately, even after making allowance for weight and head resistance of the connections.

APPENDIX C.

Letter read by Mr. Frank Lahm before the Aviation Committee of the Aero Club de France, December 29th, 1905.

Mansfield, Ohio,
December 6th, 1905.

MY DEAR LAHM,

December 1st I was at Chicago, stopping at the Grand Pacific Hotel, and after a tedious day's work, had retired early. Just as I was about falling asleep my room telephone rang, and on inquiry learned that a telegram would be sent me, and soon it was delivered to me. It was a puzzle: "Verify what Wright Brothers claim, necessary go Dayton, prompt response cable." I studied it carefully, and as the matter seemed very important, concluded to ask Wright Brothers to explain, for surely they should understand. So I telegraphed them, retired to the sleep of the just, and awaited the reply to come the next morning, and, when received, it threw no light on the subject. In desperation I telegraphed again: "Do you know Frank Lahm, of Paris?" Answer came back: "Yes, Lahm, the French aeronaut."

All this time there was dimly conscious in my mind that I knew somewhere—somehow of Wright Brothers. As soon as I saw the word "aeronaut" it came to my mind that they must be the ones who some years ago experimented with a "flying machine" in the Carolinas, and I promptly notified them I would be in Dayton the next morning and would they meet me. At seven o'clock the next morning I was there. I inquired, after a time, whether there was such a firm in the city, but no one could give me any information. There was none in the directory, and no one seemed to know anything about a "flying machine."

I then went to the telegraph office, since I knew my messages had been delivered, and finally discovered the boy who had delivered the telegrams, and through him their street address. Going back to the hotel, I found Mr. Orville Wright, the younger of the brothers, awaiting me, and about as much puzzled as I had been over what the cable message and my telegrams meant. In a few minutes we both grasped just what was wanted, and Mr. Wright promptly told

me he would do all he could to satisfy you and me that all he reported as accomplished was the truth, and nothing but the truth. His very appearance, though, would disarm any suspicions to the contrary. A young man, of about 30, apparently, slight of build, and with a face more of a poet than an inventor or promoter. In contour, head and face resemble Edgar Allen Poe. Very modest in alluding to the marvels they have accomplished—looking more to the renown and glory that will surely come to them as the solvers of the problem of mechanical flight than any possible pecuniary reward.

He told me that the place they tried their experiments was eight miles away, and we promptly sought a trolley car, and in less than an hour reached the spot. A great field of about 90 acres, flat and level, what we call meadowland, covered with coarse grass, and hummocky. Off in the distance, nearly half a mile away, was a small structure in which was housed the aeroplane during the time of their experimenting. The field was otherwise bare except three trees far to the right, and which marked in a way the limits of their circling flights.

During the preceding hour Mr. Wright had briefly outlined what they had accomplished in the season of 1905, and which closed abruptly on October 5th, for reasons to be stated later. During that time they had made some 50 flights, and the first trials had been of varying success, though with changes and improvements they were conscious that every week showed an important gain. By the middle of September they had completed the machine practically as it was when they closed the season. The frame was made of larch wood—from tip to tip of the wings the dimension was 40 feet. The gasoline motor, a special construction, made by them—much the same, though, as the motor on the Pope-Toledo automobile—was of from 12 to 15 horse-power. The motor weighed 240lbs. The frame was covered with ordinary muslin of good quality. No attempt was made to lighten the machine; they simply built it strong enough to stand the shocks. The structure stood on skids or runners, like a sleigh. These held the frame high enough from the ground in alighting to protect the blades of the propeller. Complete with motor, the machine weighed 925lbs.

The operator lies face downward in a horizontal position and as close to the frame as possible. Contrary to what would be expected, it is a fact that a weight hanging below the centre of gravity, like a pendulum, causes the aeroplane to capsize; hence all weight is concentrated as near as possible in a common plane —much as a bird flies, drawing its feet close to its body. No rudder is used. What may be called a tail extends to the rear, but is used only to change the direction up or down. The guiding of the machine, cutting figure eights, or swinging with lightning speed in great circles is accomplished otherwise.

About October 1st they had finally got their motor in such shape as to ensure good, long flights. Previous to that time flights of 10, 11, and 12 miles had been made. On October 3rd 16 miles had been made; October 4th, 21 miles; and October 5th, the record was 24¼ miles, and the flight only ceased because the supply of gasoline gave out. In this flight the older brother operated the machine. They take " turn about " in this part of the work. Mr. Wright weighs 145lbs., and an additional weight of iron, 50lbs. was lashed to the prow. The great birdlike machine rushed round the circle 29 times, and the time was 38 minutes. At times the speed, with the favouring wind, was fully a mile a minute. No wonder that some of the more mercurial spectators grew wild with excitement as it rushed by, for you must remember that all the time the machine was not over 60 feet from the earth, the average height, just clearing the tree-tops, being held. Only in turning the more abrupt corners, so to speak, was the height increased.

Mr. Wright tells me he has flown a distance of four miles and held the machine so near the earth that the greatest height was not to exceed 10 feet. This would show absolute control in a vertical plane. For obvious reasons, they kept their experiments as secret as possible. The cars on the electric line ran every 30 minutes, and so as not to attract attention, they have timed their flights between the runs, and have been very successful in avoiding observation. The

farmers living near by are the only ones who have had the opportunities of watching the experiments, and after two years of such experiments, had lost what little interest they had in them.

Mr. D. Beard lives across the Springfield road from the experimental field, and to him Mr. Wright took me first. I found him an intelligent man of about 60. He told me of the work which had gone on under the eye for so long a time, and assured me absolutely there was no question as to the flights. He had often observed them, and particularly knew of the series ending October 5th of this year. We next called on Farmer Stauffer, living half a mile farther up the road. He was a typical American farmer, with jolly face and voluble. He rents the farm which includes the field where the flights were made. On October 5th he was cutting corn in the next field east, and which is higher ground. When he noticed the aeroplane had started on its flight he remarked to his helper : " Well, the boys are at it again," and kept on cutting corn, at the same time keeping an eye on the great white form rushing about its course. " I just kept on shocking corn," he continued, " until I got down to the fence, and the durned thing was still going round. I thought it never would stop." I asked him how long he thought the flight continued, and he replied it seemed to him it was in the air for half an hour.

After this interview we returned to the city. We called on Mr. Foust, druggist, a friend of Mr. Wright. On invitation, Mr. Foust was present at the trial of October 5th, and described to me fully the operation of the machine. He timed the duration of the flight, and counted the number of times it circled the field. Mr. Foust is a young and enthusiastic man, and, though cautioned by the brothers not to divulge what he had seen, he could not restrain himself, and talked about it so effectively that the next day the field was invaded by a crowd of sightseers. The fences were lined by Kodak fiends, and reluctantly the Wright Brothers were forced to discontinue their work. They took apart their flyer and brought it back to the city. The practical work of 1905 was ended.

I next called at the pleasant home of these discoverers of the " only new thing under the sun." They live very quietly with their father, who is a clergyman. The elder brother, Wilbur, I found even quieter and less demonstrative than the younger. He looked the scholar and recluse. Neither is married. As Mr. Wright expressed it, they had not the means to support " a wife and a flying machine too."

Twelve years ago they engaged in manufacturing bicycles, but gradually went out of the business as the craze subsided. Even as boys they were of an inventive turn of mind, one of their achievements being a printing press, which, on account of its great novelty, attracted much attention. And, too, at a very early age they commenced to study the problem of mechanical flight. They have made a special study of the laws governing the flight of birds, and have compiled tables of great value relating to wind resistance as applied to aeroplanes. In this they have discovered many errors in the deductions and laws laid down by those now esteemed authorities. In these matters they have found that Lilienthal was far ahead of those who followed.

They have given much thought and time to scientifically construct the best form of motor—as to the pitch of the wings, size—and it appears to me that they know far more than others in this peculiar field. My visit was made on Sunday, and for that reason I did not see all the parties who witnessed or knew the work accomplished. I enclose you letter just received from them, which gives further references.

They told me of their correspondence with Captain Ferber, who I understand is a member of your Aero Club, and laughed at his assertion that there " was not a man in all France who believed they had done what they claimed."

(Signed) HENRY M. WEAVER.

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APPENDIX D.

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THE WRIGHT BROTHERS' AEROPLANE

BY ORVILLE AND WILBUR WRIGHT

WITH PICTURES FROM PHOTOGRAPHS SUPPLIED BY THE AUTHORS.*

[The article which follows is the first popular account of their experiments prepared by the inventors. Their accounts heretofore have been brief statements of bare accomplishments, without explanation of the manner in which results were attained. The article will be found of special interest, in view of the fact that they have contracted to deliver to the United States Government a complete machine, the trials of which are expected to take place about the time of the appearance of this number of "The Century."—THE EDITOR.]

Though the subject of aerial navigation is generally considered new, it has occupied the minds of men more or less from the earliest ages. Our personal interest in it dates from our childhood days. Late in the autumn of 1878 our father came into the house one evening with some object partly concealed in his hands, and before we could see what it was, he tossed it into the air. Instead of falling to the floor, as we expected, it flew across the room till it struck the ceiling, where it fluttered awhile, and finally sank to the floor. It was a little toy, known to scientists as a "hélicoptère," but which we, with sublime disregard for science, at once dubbed a "bat." It was a light frame of cork and bamboo, covered with paper, which formed two screws, driven in opposite directions by rubber bands under torsion. A toy so delicate lasted only a short time in the hands of small boys, but its memory was abiding.

Several years later we began building these hélicoptères for ourselves, making each one larger than that preceding. But, to our astonishment, we found that the larger the "bat" the less it flew. We did not know that a machine having only twice the linear dimensions of another would require eight times the power. We finally became discouraged, and returned to kite-flying, a sport to which we had devoted so much attention that we were regarded as experts. But as we became older we had to give up this fascinating sport as unbecoming to boys of our ages.

It was not till the news of the sad death of Lilienthal reached America in the summer of 1896 that we again gave more than passing attention to the subject of flying. We then studied with great interest Chanute's "Progress in Flying Machines," Langley's "Experiments in Aërodynamics," the "Aëronautical Annuals" of 1905, 1906, and 1907, and several pamphlets published by the Smithsonian Institution, especially articles by Lilienthal and extracts from Mouillard's "Empire of the Air." The larger works gave us a good understanding of the nature of the flying problem, and the difficulties in past attempts to solve it, while Mouillard and Lilienthal, the great missionaries of the flying cause, infected us with their own unquenchable enthusiasm, and transformed idle curiosity into the active zeal of workers.

In the field of aviation there were two schools. The first, represented by such men as Professor Langley and Sir Hiram Maxim, gave chief attention to

* Not reproduced here.

power flight; the second, represented by Lilienthal, Mouillard, and Chanute, to soaring flight. Our sympathies were with the latter school, partly from impatience at the wasteful extravagance of mounting delicate and costly machinery on wings which no one knew how to manage, and partly, no doubt, from the extraordinary charm and enthusiasm with which the apostles of soaring flight set forth the beauties of sailing through the air on fixed wings, deriving the motive power from the wind itself.

The balancing of a flyer may seem, at first thought, to be a very simple matter, yet almost every experimenter had found in this one point which he could not satisfactorily master. Many different methods were tried. Some experimenters placed the centre of gravity far below the wings, in the belief that the weight would naturally seek to remain at the lowest point. It is true that, like the pendulum, it tended to seek the lowest point; but also, like the pendulum, it tended to oscillate in a manner destructive of all stability. A more satisfactory system, especially for lateral balance, was that of arranging the wings in the shape of a broad V, to form a dihedral angle, with the centre low and the wing-tips elevated. In theory this was an automatic system, but in practice it had two serious defects: first, it tended to keep the machine oscillating; and second, its usefulness was restricted to calm air.

In a slightly modified form the same system was applied to the fore-and-aft balance. The main aeroplane was set at a positive angle, and a horizontal tail at a negative angle, while the centre of gravity was placed far forward. As in the case of lateral control, there was a tendency to constant undulation, and the very forces which caused a restoration of balance in calms caused a disturbance of the balance in winds. Notwithstanding the known limitations of this principle, it had been embodied in almost every prominent flying machine which had been built.

After considering the practical effect of the dihedral principle, we reached the conclusion that a flyer founded upon it might be of interest from a scientific point of view, but could be of no value in a practical way. We therefore resolved to try a fundamentally different principle. We would arrange the machine so that it would not tend to right itself. We would make it as inert as possible to the effects of change of direction or speed, and thus reduce the effects of wind-gusts to a minimum. We would do this in the fore-and-aft stability by giving the aeroplanes a peculiar shape; and in the lateral balance by arching the surfaces from tip to tip, just the reverse of what our predecessors had done. Then by some suitable contrivance, actuated by the operator, forces should be brought into play to regulate the balance.

Lilienthal and Chanute had guided and balanced their machines by shifting the weight of the operator's body. But this method seemed to us incapable of expansion to meet large conditions, because the weight to be moved and the distance of possible motion were limited, while the disturbing forces steadily increased, both with wing area and with wind velocity. In order to meet the needs of large machines, we wished to employ some system whereby the operator could vary at will the inclination of different parts of the wings, and thus obtain from the wind forces to restore the balance which the wind itself had disturbed. This could easily be done by using wings capable of being warped, and by supplementary adjustable surfaces in the shape of rudders. As the forces obtainable for control would necessarily increase in the same ratio as the disturbing forces, the method seemed capable of expansion to an almost unlimited extent. A happy device was discovered whereby the apparently rigid system of superposed surfaces, invented by Wenham, and improved by Stringfellow and Chanute, could be warped in a most unexpected way, so that the aeroplanes could be presented on the right and left sides at different angles to the wind. This, with an adjustable, horizontal front rudder, formed the main feature of our first glider.

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The period from 1885 to 1900 was one of unexampled activity in aeronautics, and for a time there was high hope that the age of flying was at hand. But Maxim, after spending $100,000, abandoned the work; the Ader machine, built at the expense of the French Government, was a failure; Lilienthal and Pilcher were killed in experiments; and Chanute and many others, from one cause or another, had relaxed their efforts, though it subsequently became known that Professor Langley was still secretly at work on a machine for the United States Government. The public, discouraged by the failures and tragedies just witnessed, considered flight beyond the reach of man, and classed its adherents with the inventors of perpetual motion.

We began our active experiments at the close of this period, in October, 1900, at Kitty Hawk, North Carolina. Our machine was designed to be flown as a kite, with a man on board, in winds of from 15 to 20 miles an hour. But, upon trial, it was found that much stronger winds were required to lift it. Suitable winds not being plentiful, we found it necessary, in order to test the new balancing system, to fly the machine as a kite without a man on board, operating the levers through cords from the ground. This did not give the practice anticipated, but it inspired confidence in the new system of balance.

In the summer of 1901 we became personally acquainted with Mr. Chanute. When he learned that we were interested in flying as a sport, and not with any expectation of recovering the money we were expending on it, he gave us much encouragement. At our invitation, he spent several weeks with us at our camp at Kill Devil Hill, four miles south of Kitty Hawk, during our experiments of that and the two succeeding years. He also witnessed one flight of the power machine near Dayton, Ohio, in October, 1904.

The machine of 1901 was built with the shape of surface used by Lilienthal, curved from front to rear like the segment of a parabola, with a curvature 1/12 the depth of its cord; but to make doubly sure that it would have sufficient lifting capacity when flown as a kite in 15 or 20-mile winds, we increased the area from 165 square feet, used in 1900, to 308 square feet—a size much larger than Lilienthal, Pilcher, or Chanute had deemed safe. Upon trial, however, the lifting capacity again fell very far short of calculation, so that the idea of securing practice while flying as a kite had to be abandoned. Mr. Chanute, who witnessed the experiments, told us that the trouble was not due to poor construction of the machine. We saw only one other explanation—that the tables of air-pressures in general use were incorrect.

We then turned to gliding—coasting downhill on the air—as the only method of getting the desired practice in balancing a machine. After a few minutes' practice we were able to make glides of over 300 feet, and in a few days were safely operating in 27-mile* winds. In these experiments we met with several unexpected phenomena. We found that, contrary to the teachings of the books, the centre of pressure on a curved surface travelled backward when the surface was inclined, at small angles, more and more edgewise to the wind. We also discovered that in free flight, when the wing on one side of the machine was presented to the wind at a greater angle than the one on the other side, the wing with the greater angle descended, and the machine turned in a direction just the reverse of what we were led to expect when flying the machine as a kite. The larger angle gave more resistance to forward motion, and reduced the speed of the wing on that side. The decrease in speed more than counterbalanced the effect of the larger angle. The addition of a fixed vertical vane in the rear increased the trouble, and made the machine absolutely dangerous. It was some time before a remedy was discovered. This consisted of movable rudders working

* The gliding flights were all made against the wind. The difficulty in high winds is in maintaining balance, not in travelling against the wind.

in conjunction with the twisting of the wings. The details of this arrangement are given in our patent specifications, published several years ago.

The experiments of 1901 were far from encouraging. Although Mr. Chanute assured us that, both in control and in weight carried per horse-power, the results obtained were better than those of any of our predecessors, yet we saw that the calculations upon which all flying machines had been based were unreliable, and that all were simply groping in the dark. Having set out with absolute faith in the existing scientific data, we were driven to doubt one thing after another, till finally, after two years of experiment, we cast it all aside, and decided to rely entirely upon our own investigations. Truth and error were everywhere so intimately mixed as to be undistinguishable. Nevertheless, the time expended in preliminary study of books was not misspent, for they gave us a good general understanding of the subject, and enabled us at the outset to avoid effort in many directions in which results would have been hopeless.

The standard for measurements of wind-pressures is the force produced by a current of air of one mile per hour velocity striking square against a plane of one square foot area. The practical difficulties of obtaining an exact measurement of this force have been great. The measurements by different recognised authorities vary 50 per cent. When this simplest of measurements presents so great difficulties, what shall be said of the troubles encountered by those who attempt to find the pressure at each angle as the plane is inclined more and more edgewise to the wind? In the eighteenth century the French Academy prepared tables giving such information, and at a later date the Aëronautical Society of Great Britain made similar experiments. Many persons likewise published measurements and formulas; but the results were so discordant that Professor Langley undertook a new series of measurements, the results of which form the basis of his celebrated work, "Experiments in Aërodynamics." Yet a critical examination of the data upon which he based his conclusions as to the pressures at small angles shows results so various as to make many of his conclusions little better than guesswork.

To work intelligently, one needs to know the effects of a multitude of variations that could be incorporated in the surfaces of flying machines. The pressures on squares are different from those on rectangles, circles, triangles, or ellipses; arched surfaces differ from planes, and vary among themselves according to the depth of curvature; true arcs differ from parabolas, and the latter differ among themselves; thick surfaces differ from thin, and surfaces thicker in one place than another vary in pressure when the positions of maximum thickness are different; some surfaces are most efficient at one angle, others at other angles. The shape of the edge also makes a difference, so that thousands of combinations are possible in so simple a thing as a wing.

We had taken up aeronautics merely as a sport. We reluctantly entered upon the scientific side of it. But we soon found the work so fascinating that we were drawn into it deeper and deeper. Two testing machines were built, which we believed would avoid the errors to which the measurements of others had been subject. After making preliminary measurements on a great number of different-shaped surfaces, to secure a general understanding of the subject, we began systematic measurements of standard surfaces, so varied in design as to bring out the underlying causes of differences noted in their pressures. Measurements were tabulated on nearly 50 of these at all angles from zero to 45 degrees, at intervals of 2½ degrees. Measurements were also secured showing the effects on each other when surfaces are superposed, or when they follow one another.

Some strange results were obtained. One surface, with a heavy roll at the front edge, showed the same lift for all angles from 7½ to 45 degrees. A square plane, contrary to the measurements of all our predecessors, gave a greater pressure at 30 degrees than at 45 degrees. This seemed so anomalous that we were almost ready to doubt our own measurements, when a simple test was suggested.

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A weather-vane, with two planes attached to the pointer at an angle of 80 degrees with each other, was made. According to our tables, such a vane would be in unstable equilibrium when pointing directly into the wind; for if by chance the wind should happen to strike one plane at 39 degrees and the other at 41 degrees, the plane with the smaller angle would have the greater pressure, and the pointer would be turned still farther out of the course of the wind until the two vanes again secured equal pressures, which would be at approximately 30 and 50 degrees. But the vane performed in this very manner. Further corroboration of the tables was obtained in experiments with the new glider at Kill Devil Hill the next season.

In September and October, 1902, nearly 1,000 gliding flights were made, several of which covered distances of over 600 feet. Some, made against a wind of 36 miles an hour, gave proof of the effectiveness of the devices for control. With this machine, in the autumn of 1903, we made a number of flights in which we remained in the air for over a minute, often soaring for a considerable time in one spot, without any descent at all. Little wonder that our unscientific assistant should think the only thing needed to keep it indefinitely in the air would be a coat of feathers to make it light!

With accurate data for making calculations, and a system of balance effective in winds as well as in calms, we were now in a position, we thought, to build a successful power-flyer. The first designs provided for a total weight of 600lbs., including the operator and an eight horse-power motor. But, upon completion, the motor gave more power than had been estimated, and this allowed 150lbs. to be added for strengthening the wings and other parts.

Our tables made the designing of the wings an easy matter, and as screw-propellers are simply wings travelling in a spiral course, we anticipated no trouble from this source. We had thought of getting the theory of the screw-propeller from the marine engineers, and then, by applying our tables of air-pressures to their formulas, of designing air-propellers suitable for our purpose. But so far as we could learn, the marine engineers possessed only empirical formulas, and the exact action of the screw-propeller, after a century of use, was still very obscure. As we were not in a position to undertake a long series of practical experiments to discover a propeller suitable for our machine, it seemed necessary to obtain such a thorough understanding of the theory of its reactions as would enable us to design them from calculation alone. What at first seemed a simple problem became more complex the longer we studied it. With the machine moving forward, the air flying backward, the propellers turning sidewise, and nothing standing still, it seemed impossible to find a starting-point from which to trace the various simultaneous reactions. Contemplation of it was confusing. After long arguments we often found ourselves in the ludicrous position of each having been converted to the other's side, with no more agreement than when the discussion began.

It was not till several months had passed, and every phase of the problem had been thrashed over and over, that the various reactions began to untangle themselves. When once a clear understanding had been obtained there was no difficulty in designing suitable propellers, with proper diameter, pitch, and area of blade, to meet the requirements of the flyer. High efficiency in a screw-propeller is not dependent upon any particular or peculiar shape, and there is no such thing as a "best" screw. A propeller giving a high dynamic efficiency when used upon one machine may be almost worthless when used upon another. The propeller should in every case be designed to meet the particular conditions of the machine to which it is to be applied. Our first propellers, built entirely from calculation, gave in useful work 66 per cent. of the power expended. This was about one-third more than had been secured by Maxim or Langley.

The first flights with the power machine were made on December 17, 1903. Only five persons besides ourselves were present. These were Messrs. John T.

Daniels, W. S. Dough, and A. D. Etheridge, of the Kill Devil Life-Saving Station; Mr. W. C. Brinkley, of Manteo; and Mr. John Ward, of Naghead. Although a general invitation had been extended to the people living within five or six miles, not many were willing to face the rigours of a cold December wind in order to see, as they no doubt thought, another flying machine *not* fly. The first flight lasted only 12 seconds, a flight very modest compared with that of birds, but it was, nevertheless, the first in the history of the world in which a machine carrying a man had raised itself by its own power into the air in free flight, had sailed forward on a level course without reduction of speed, and had finally landed without being wrecked. The second and third flights were a little longer, and the fourth lasted 59 seconds, covering a distance of 852 feet over the ground against a 20-mile wind.

After the last flight the machine was carried back to camp and set down in what was thought to be a safe place. But a few minutes later, while we were engaged in conversation about the flights, a sudden gust of wind struck the machine, and started to turn it over. All made a rush to stop it, but we were too late. Mr. Daniels, a giant in stature and strength, was lifted off his feet, and falling inside, between the surfaces, was shaken about like a rattle in a box as the machine rolled over and over. He finally fell out upon the sand with nothing worse than painful bruises, but the damage to the machine caused a discontinuance of experiments.

In the spring of 1904, through the kindness of Mr. Torrence Huffman, of Dayton, Ohio, we were permitted to erect a shed, and to continue experiments, on what is known as the Huffman Prairie, at Simms Station, eight miles east of Dayton. The new machine was heavier and stronger, but similar to the one flown at Kill Devil Hill. When it was ready for its first trial every newspaper in Dayton was notified, and about a dozen representatives of the Press were present. Our only request was that no pictures be taken, and that the reports be unsensational, so as not to attract crowds to our experiment grounds. There were probably 50 persons altogether on the ground. When preparations had been completed a wind of only three or four miles was blowing—insufficient for starting on so short a track—but since many had come a long way to see the machine in action, an attempt was made. To add to the other difficulty, the engine refused to work properly. The machine, after running the length of the track, slid off the end without rising into the air at all. Several of the newspaper men returned the next day, but were again disappointed. The engine performed badly, and after a glide of only 60 feet, the machine came to the ground. Further trial was postponed till the motor could be put in better running condition. The reporters had now, no doubt, lost confidence in the machine, though their reports, in kindness, concealed it. Later, when they heard that we were making flights of several minutes' duration, knowing that longer flights had been made with airships, and not knowing any essential difference between airships and flying machines, they were but little interested.

We had not been flying long in 1904 before we found that the problem of equilibrium had not as yet been entirely solved. Sometimes, in making a circle, the machine would turn over sidewise despite anything the operator could do, although, under the same conditions in ordinary straight flight, it could have been righted in an instant. In one flight, in 1905, while circling around a honey locust-tree at a height of about 50 feet, the machine suddenly began to turn up on one wing, and took a course toward the tree. The operator, not relishing the idea of landing in a thorn-tree, attempted to reach the ground. The left wing, however, struck the tree at a height of 10 or 12 feet from the ground and carried away several branches; but the flight, which had already covered a distance of six miles, was continued to the starting-point.

The causes of these troubles—too technical for explanation here—were not entirely overcome till the end of September, 1905. The flights then rapidly in-

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creased in length, till experiments were discontinued after October 5, on account of the number of people attracted to the field. Although made on a ground open on every side, and bordered on two sides by much-travelled thoroughfares, with electric cars passing every hour, and seen by all the people living in the neighbourhood for miles around, and by several hundred others, yet these flights have been made by some newspapers the subject of a great "mystery."

A practical flyer having been finally realised, we spent the years 1906 and 1907 in constructing new machines and in business negotiations. It was not till May of this year that experiments (discontinued in October, 1905) were resumed at Kill Devil Hill, North Carolina. The recent flights were made to test the ability of our machine to meet the requirements of a contract with the United States Government to furnish a flyer capable of carrying two men and sufficient fuel supplies for a flight of 125 miles, with a speed of 40 miles an hour. The machine used in these tests was the same one with which the flights were made at Simms Station in 1905, though several changes had been made to meet present requirements. The operator assumed a sitting position, instead of lying prone, as in 1905, and a seat was added for a passenger. A larger motor was installed, and radiators and gasolene reservoirs of larger capacity replaced those previously used. No attempt was made to make high or long flights.

In order to show the general reader the way in which the machine operates, let us fancy ourselves ready for the start. The machine is placed upon a single-rail track facing the wind, and is securely fastened with a cable. The engine is put in motion, and the propellers in the rear whir. You take your seat at the centre of the machine beside the operator. He slips the cable, and you shoot forward. An assistant who has been holding the machine in balance on the rail starts forward with you, but before you have gone 50 feet the speed is too great for him, and he lets go. Before reaching the end of the track the operator moves the front rudder, and the machine lifts from the rail like a kite supported by the pressure of the air underneath it. The ground under you is at first a perfect blur, but as you rise the objects become clearer. At a height of 100 feet you feel hardly any motion at all, except for the wind which strikes your face. If you did not take the precaution to fasten your hat before starting, you have probably lost it by this time. The operator moves a lever: the right wing rises, and the machine swings about to the left. You make a very short turn, yet you do not feel the sensation of being thrown from your seat, so often experienced in automobile and railway travel. You find yourself facing toward the point from which you started. The objects on the ground now seem to be moving at much higher speed, though you perceive no change in the pressure of the wind on your face. You know then that you are travelling with the wind. When you near the starting-point the operator stops the motor while still high in the air. The machine coasts down at an oblique angle to the ground, and after sliding 50 or 100 feet, comes to rest. Although the machine often lands when travelling at a speed of a mile a minute, you feel no shock whatever, and cannot, in fact, tell the exact moment at which it first touched the ground. The motor close beside you kept up an almost deafening roar during the whole flight, yet in your excitement you did not notice it till it stopped!

Our experiments have been conducted entirely at our own expense. In the beginning we had no thought of recovering what we were expending, which was not great, and was limited to what we could afford for recreation. Later, when a successful flight had been made with a motor, we gave up the business in which we were engaged, to devote our entire time and capital to the development of a machine for practical uses. As soon as our condition is such that constant attention to business is not required, we expect to prepare for publication the results of our laboratory experiments, which alone made an early solution of the flying problem possible.

APPENDIX E.

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WHAT MOUILLARD DID.

BY WILBUR WRIGHT.

[Until a few months ago the name of Louis Pierre Mouillard was known to only a few deep students of aeronautics and to them he appeared as an elusive personality, a French student—farmer—poet—who had lived in Egypt and had written a remarkable book upon bird flight. Of his life and the value of his work nothing definite was known. Two years ago, during the Heliopolis meet, somebody remembered that Mouillard had lived and died at Cairo and started an investigation, which ended with the finding of a boxful of papers of Mouillard in the cellars of the French Consulate, where they had been stored on the death of Mouillard. These papers were acquired by an officer of the Ligue Aerienne who undertook to edit them. From time to time since then the Ligue has advanced for Mouillard the claim of his having discovered the principle of wing warping. Because there were found among the Mouillard papers letters of Chanute showing that in their correspondence he and Mouillard had discussed the peculiarities of bird flight, and in particular a belief of Mouillard's that birds turn by creating a resistance at the tip of the wing upon the side toward which they wish to go, it has been sought to intimate that the principle of warping was revealed by Mouillard to Chanute, and by him was communicated to the Wrights. These claims have never been advanced officially, nor substantiated with proof. To clear this point, for historical purposes, we asked the Wright brothers to define both Mouillard's place in history and the value of his contribution. The following article by Wilbur Wright is made especially timely by the inauguration of a monument to Mouillard at Heliopolis, February 25, 1912.—EDITOR.]

The erection at Cairo, Egypt, of a monument to L. P. Mouillard, recalls attention to one of the greatest missionaries of the flying cause which the nineteenth century produced. Mouillard was a Frenchman who passed a large part of his life in Algeria and Egypt, where his attention was attracted by the wonderful soaring of vultures on fixed wings. His imagination was greatly excited by what he saw, and during the remainder of his life he was like a prophet crying in the wilderness, exhorting the world to repent of its unbelief in the possibility of human flight. In 1881 he published a book called "The Empire of the Air," which is one of the most remarkable pieces of aeronautical literature that has ever been published. In his introduction he says:—

> "If there be a domineering, tyrant thought, it is the conception that the problem of flight may be solved by man. When once this idea has invaded the brain it possesses it exclusively. It is then a haunting thought, a walking nightmare, impossible to cast off. If now we consider the pitying contempt with which such a line of research is appreciated, we may somewhat conceive the unhappy lot of the poor investigator whose soul is thus possessed."

He deplores the incredulity of the world and exhorts it to cast aside its unbelief:—

> "O! blind humanity! open thine eyes and thou shalt see millions of birds and myriads of insects cleaving the atmosphere. All these creatures are whirling through the air without the slightest float; many of them are gliding therein, without losing height, hour after hour, on pulseless wings without fatigue; and after beholding this demonstration, given by the source of all knowledge, thou wilt acknowledge that Aviation is the path to be followed."

His observations upon the habits of vultures led him to the conclusion that flight without motors was possible to man, and this idea he presented to his readers with an enthusiasm so inspiring and convincing that his book produced results of the greatest importance in the history of flight. The man was himself almost fanatical in his enthusiasm. Speaking of his first sight of a vulture in full soaring flight, he says:—

> "All my life shall I remember the first flight which I saw of the Gyps

fulvus, the great tawny vultures of Africa. I was so impressed that all day long I could think of nothing else; and indeed there was good cause, for it was a practical, perfect demonstration of all my preconceived theories concerning the possibilities of artificial flight in a wind. Since then I have observed thousands of vultures. I have disturbed many of the vast flocks of these birds, and yet, even now, I cannot see one individual passing through the air without following him with my eyes until he disappears in the distant horizon.

"The vulture's needs are few, and his strength is moderate. To earn his living he but needs to sight the dead animal from afar. And so what does he know? He knows how to rise, how to float aloft, to sweep the field with keen vision, to sail upon the wind without effort, till the carcass is seen, and then to descend slowly, after careful reconnaissance and assurance that he may alight without danger, that he will not be surprised, and compelled to precipitous and painful departure. And so he has evolved a peculiar mode of flight; he sails and spends no force, he never hurries, he uses the wind instead of his muscles, and the wing flap occasionally seen is meant to limber up rather than to hasten through the air. And so the true model to study is the vulture—the great vulture. Beside him the stork is as a wren, the kite a mere butterfly, the falcon a pin feather.

"Whoso has for five minutes had the fortune to see the oricou vulture in full sail through the air, and has not perceived the possibility of his imitation by man, is—I will not say of dull understanding, but certainly inapt to analyse and to appreciate."

Throughout the book are to be found passages of high literary quality, and the charm is so great that more than one cold-blooded reader has been incited to emulate the example of the birds. There is no doubt that the reading of this book was one of the main factors in inducing Mr. Chanute to undertake his experiments, and I know that it was one of the inspiring causes of the efforts of the Wright brothers. Compared with this book, which is devoted almost entirely to observations relating to birds, the ordinary books on ornithology are childish. With the possible exception of Lilienthal, none of the men who wrote on aviation in the nineteenth century possessed such power to draw recruits to a belief in the possibility of motorless human flight.

As a missionary, Mouillard stood at the very top along with Lilienthal and Chanute. As a scientific student of the laws and principles of aerodynamics he is not to be mentioned in the same class with such men as Cayley, Wenham, Penaud, Langley, Lilienthal, Chanute, and Maxim. He was a careful observer of birds, and possessed a genius for expressing his thoughts and feelings in words, but beyond that he was mediocre. He made a few feeble attempts to construct soaring machines, but their design and construction were so crude that he failed to surpass the futile attempts at gliding made by Cayley and Wenham, who long antedated him. It remained for Lilienthal to definitely employ this mode of experiment, and thereby win for himself a glory which the world will never forget.

It is most unfortunate that the project of erecting a monument to a man well worthy of the thanks and the remembrance of the world should have become entangled with an unworthy attempt to seek to add to the glory of France by filching the credit justly due to Lilienthal, and by falsely accusing Mr. Chanute, the benefactor of Mouillard, of having stolen the latter's secrets and transmitted them to the Wright brothers. There is in France a little group of misguided individuals who bring disgrace upon their country by their too zealous attempts to add to its glory. Fortunately, they do not represent the real France, which has shown by numerous manifestations of various kinds its high appreciation of the work of foreigners, including even Lilienthal, a native of a country greatly disliked by French people.

This group some years ago formed a society known as the "Ligue Aerienne," and made it their purpose to convince themselves and the world that France was the birthplace of human flight. To begin with, they hailed Santos Dumont as the "father of aviation," because of his flights in 1906. When it was proved that flights had been made outside of France long before that they then fell back on Ader and hailed him as the "father of aviation," on account of a mythical flight in 1897. But when the Minister of War permitted the publication of the official report on the trial of this machine, which showed that it had never left the ground, but had been wrecked while running along the track with only a small part of the power turned on, this indefatigable group went back to Mouillard and proceeded to erect a monument to him as the "father of aviation." If they had been content with this, their activity might have been passed with a smile, but when in addition they attempted to pervert history and accuse Mr. Chanute of dishonestly getting from Mouillard the secret of warping the wings to control lateral balance, and transmitting it to the Wright brothers, it is well to expose their errors.

The position of Lilienthal as the founder of gliding experiments is too fully established to make it necessary to defend it here. The facts are well known.

The fact that the Wright brothers had been using wing warping several years before Mr. Chanute became acquainted with them effectually disposes of the part of the story accusing Mr. Chanute of transmitting any of Mouillard's secret to them. The fact that Mouillard never had the idea of warping the wings to control lateral balance, and never communicated such an idea to Mr. Chanute, is also sufficient of itself to refute the charge.

It only remains to discuss that part accusing Mr. Chanute of having received from Mouillard the manuscript of a proposed book and, after reading it, of having dishonestly advised him to suppress it. It is quite clear that the men who manufactured this shameful attack on the memory of a dead man had never read Mr. Chanute's book, "Progress in Flying Machines," published in 1893. This book contains the fullest and most appreciative account of Mouillard which is to be found in the literature of flying. It discusses not only his book, "The Empire of the Air," and his attempts to construct apparatus, but also makes copious extracts, with full credit to Mouillard, from the manuscript which these ignoramuses accuse him of trying to keep from the knowledge of the world. Not only that, but Mr. Chanute, learning that Mouillard was an invalid and without funds, furnished the money to secure a United States patent on his invention, and gave him a considerable sum of money in addition. The patent issued and was published in 1897. Mr. Chanute never received a cent of benefit from it. *In this patent Mouillard proposed to make the right and left rear corners of the wings double, so that one or the other could be distorted to create a resistance in order to turn the machine to right or left. No vertical tail was to be used. The subject of lateral balance was not even mentioned.*

The Mouillard patent was cited by the defendants in the case of The Wright Company v. Louis Paulhan, and Judge Hand, in his decision, refers specifically to it, and says, "In no one of the 19 claims is there anything which in any way even foreshadows the patent [of the Wright brothers] in suit." Mr. Chanute's book and the patent clearly show that he made every effort to spread the fame and improve the finances of Mouillard.

The memory of Mouillard is well deserving of perpetuation by a monument, but it is a pity that it should have been used by a self-constituted group of pretended champions of French glory, in a disgraceful Chauvinistic campaign of slander and detraction not approved by the mass of the French people.

[The following excerpt from the Mouillard patent, No. 582,757, of 1897, which was owned jointly by Mouillard and Octave Chanute, fully elucidates the foregoing article by Mr. Wilbur Wright.—EDITOR.]

"In order to provide for the horizontal steering of the apparatus—that is, the guiding it to the right or left—I substitute for the ordinary rudder a

novel and more effective arrangement. A portion (J) of the fabric at the rear of each wing is free from the frame at its outer edge and at the sides. It is stiffened with suitable blades or slats (N), of flexible material, and normally rests up against the netting. Cords (O) are attached to the rear edge of the portion (J') and pass forward to rings (P), where they unite and run to the handles (Q) near the inner ends of the wings. A pull upon one of these handles causes the portion (J') to curve downward (as shown in Fig. 10), *and thus catch the air, increasing the resistance upon that side of the apparatus* and causing it to turn in that direction. Any other equivalent device for creating at will an additional resistance to the air on either side of the apparatus may be employed, and I do not limit myself to the one shown and described. . . .

"The horizontal steering is effected by the downwardly movable rear portion (J') of the fabric in the manner already described. When both sides are pulled down together they serve as an effective brake to check the speed."

APPENDIX F.

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WHAT CLEMENT ADER DID.

BY WILBUR WRIGHT.

[Who was the first man to leave the ground in a self-propelled heavier-than-air craft? is a question that has been asked over and over since human flight became an actuality and has elicited considerable discussion. The Wright brothers are, of course, known to have made the first real flight, but the full honour has never been accorded them in some quarters on account of rumours which credited Clement Ader, the French pioneer, with first leaving contact with the ground during a trial of the machine which he constructed for the French Government in the year 1897. The experiments of Ader were made in secret, but the outcome being unsatisfactory were discontinued. Nothing was heard about the matter thereafter until three years ago when some French patriots advanced for Ader the claim of having been the man who first broke the shackles of gravity in a power-driven aeroplane. This claim was investigated by French aeronautical authorities, who obtained from the French Government the official report of the Ader experiment, and finding that the claim was not clear rejected it. But as is often the case with matters of similar nature, while the first claim was published broadcast, the final outcome was known only to the few directly concerned, so that so far as the public is concerned, the Ader claim still stands unsettled; and it is not unusual to find in books statements crediting Ader with the feat. The following authoritative article by Mr. Wilbur Wright is therefore of historical value. It is the first comprehensive presentation of the subject and the first definition of Ader's place in the history of aviation ever written.—EDITOR.]

Clement Ader was a French electrical engineer, who during the last quarter of the nineteenth century devoted a great deal of study and money to the problem of human flight. In his youth he had become interested in the flight of birds, and at a later period had made a study of bats, with a view to imitating their structure in a flying machine. Between the years 1882 and 1890 he built a number of pieces of apparatus, of which the last, finished in 1890, was fitted with a steam engine.

Having exhausted his own resources, he succeeded in enlisting the aid of the French Government, and proceeded to the construction of a large machine, having a steam motor of 40 horse-power. This apparatus was tried under conditions of great secrecy in October, 1897, at the military field at Satory, near Paris, in the presence of a Commission representing the French Government, but the results were so unsatisfactory that the French Government, which had spent more than

$100,000 on the project, refused to advance further funds, and Ader abandoned the attempt to solve the problem.

Ader did not leave the Ground.

After the possibility of human flight had been demonstrated by the Wright brothers, claims began to be made that the Ader machine, before being wrecked, had flown nearly 1,000 feet in 1897. After a time a systematic attempt was begun to establish by constant repetition a legend which might eventually displace the truth. But the friends of M. Santos Dumont, who claimed the honour of being the first man to fly within the borders of France, became aroused and a heated controversy arose.

At length M. Archdeacon succeeded in obtaining permission of the French Ministry of War to publish the official report of the Commission which supervised the trials of the Ader machine in 1897. The report, which conclusively shows that the Ader machine never left the ground, was as follows:—

GENERAL MENSIER'S REPORT.

Paris, October 21, 1897.

Report of the Trials of Mr. Clement Ader's Aviation Apparatus.

Mr. Ader having notified the Minister of War by letter, July 21, 1897, that the Apparatus of Aviation which he had agreed to build under the conditions set forth in the Convention of July 24, 1894, was ready, and therefore requesting that trials be undertaken before a Committee appointed for this purpose as per the decision of August 4, the Committee was appointed as follows:—

Division General Mensier, Chairman; Division General Delambre, Inspector General of the Permanent Works for Coast Defence, Member of the Tehcnical Committee of the Engineering Corps; Colonel Laussedat, Director of the Conservatoire des Arts et Metiers; Sarrau, Member of the Institute, Professor of Mechanical Engineering at the Polytechnique School; Leaute, Member of the Institute, Professor of Mechanical Engineering at the Polytechnique School.

Colonel Laussedat gave notice at once that his health and work as Director of the Conservatoire des Arts et Métiers did not permit him to be a member of the Committee; the Minister therefore accepted his resignation September 24, and decided not to replace him.

Later on, however, on the request of the Chairman of the Committee, the Minister appointed a new member, General Grillon, commanding the Engineer Corps of the Military Government of Paris.

To carry on the trials which were to take place at the Camp of Satory, the Minister ordered the Governor of the Military forces of Paris to requisition from the Engineer Corps, on request of the Chairman of the Committee, the men necessary to prepare the grounds at Satory.

After an inspection made on the 16th an aerodrome was chosen. Mr. Ader's idea was to have it of circular shape with a width of 40 metres and an average diameter of 450 metres. The preliminary work, laying out the grounds, interior and exterior circumference, etc., was finished at the end of August; the work of smoothing off the grounds began September 1 with 45 men and two rollers and was finished on the day of the first tests October 12.

The first meeting of the Committee was held August 18 in Mr. Ader's workshop; the object being to demonstrate the machine to the Committee and give the members all the information possible on the tests that were to be held. After a careful examination and after having heard all the explanations by the inventor which were deemed useful and necessary, the Committee decided that the apparatus seemed to be built with a perfect understanding of the purpose to be fulfilled as far as one could judge from the study of the apparatus at rest; they therefore authorised Mr. Ader to take the machine apart and carry it to the Camp at Satory so as to proceed with the trials.

By letter of August 19 the Chairman made a report to the Minister of the findings of the Committee.

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The work on the grounds having taken longer than was anticipated, the Chairman took advantage of this delay to call the Committee together for a second meeting during which Mr. Ader was to run the two propulsive screws situated at the forward end of the apparatus.

The meeting was held October 2; it gave the Committee an opportunity to appreciate the motive power in all its details; firebox, boiler, engine, under perfect control, absolute condensation, automatic fuel and feed of the liquid to be vaporised, automatic lubrication and scavenging; everything in a word seemed well designed and executed.

The weights in comparison with the power of the engine realised a considerable advance over anything made to date, since the two engines weighed together 42 kg., the firebox and boiler 60 kg., the condenser 15 kg., or a total of 117 kg. for approximately 40 horse-power or a little less than 3 kg. per horse-power.

One of the members summed up the general opinion by saying: "Whatever may be the result from an aviation point of view, a result which could not be foreseen for the moment, it was nevertheless proven that from a mechanical point of view Mr. Ader's apparatus was of the greatest interest and of real ingeniosity. He expressed a hope that in any case the machine would not be lost to science."

The second experiment in the workshop was made in the presence of the Chairman, the purpose being to demonstrate that the wings, having a spread of 17 metres, were sufficiently strong to support the weight of the apparatus. With this aim in view, 14 sliding supports were placed under each one of these, representing imperfectly the manner in which the wings would support the machine in the air; by gradually raising the supports with the slides, the wheels on which the machine rested were lifted from the ground. It was evident at that time that the members composing the skeleton of the wings supported the apparatus, and it was quite evident that when the wings were supported by the air on every point of their surface the stresses thereon would be better equalised than when resting upon a few supports, and that therefore the resistance to breakage would be considerably greater.

After this last test, the work on the grounds being practically finished, the machine was transported to Satory, assembled again and made ready for trial.

At first Mr. Ader was to manœuvre the machine on the grounds at a moderate speed, then increase this until it was possible to judge whether there was a tendency for the machine to rise; and it was only after Mr. Ader had acquired sufficient practice that a meeting of the Committee was to be called to be present at the first part of the trials; namely, volutions of the apparatus on the ground.

The first test took place on Tuesday, October 12, in the presence of the Chairman of the Committee. It had rained a great deal during the preceding night and the clay track would have offered considerable resistance to the rolling of the machine; furthermore, a moderate wind was blowing from the south-west, too strong during the early part of the afternoon to allow for any trials.

Toward sunset, however, the wind having weakened, Mr. Ader decided to make his first trial; the machine was taken out of its hangar, the wings were mounted and steam raised. Mr. Ader in his seat had, on each side of him, one man to the right and one to the left, whose duty was to rectify the direction of the apparatus in the event that the action of the rear wheel as a rudder would not be sufficient to hold the machine in a straight course.

At 5.25 p.m. the machine was started, first slowly then at an increased speed; after 250 or 300 metres, the two men who were being dragged by the apparatus were exhausted and forced to fall flat on the ground in order to allow the wings to pass over them and the trip around the track was completed, or a total of 1,400 metres, without incident at a fair speed, which could be estimated to be from 300 to 400 metres per minute. Notwithstanding Mr. Ader's inexperience, this being the first time that he had run his apparatus, he followed approximately the chalk line which marked the centre of the track and he stopped at the exact point from which he started.

The marks of the wheels on the grounds, which were rather soft, did not show up very much, and it was clear that a part of the weight of the apparatus had been supported by the wings, though the speed was only about one-third of what the machine could do, had Mr. Ader used all its motive power; he was running at a pressure of from 3 to 4 atmospheres, when he could have used ten or twelve.

This first trial, so fortunately accomplished, was of great importance; it was the first time that a comparatively heavy vehicle (nearly 400 kg. including the weight of the operator, fuel and water) had been set in motion by a tractive apparatus, using the air solely as a propelling medium. The favourable report turned in by the Committee after the meeting of October 2 was found justified by the results demonstrated on the grounds, and the first problem of aviation, namely, the creation of efficient motive power, could be considered as solved, since the propulsion of the apparatus in the air would be a great deal easier than the traction on the ground, provided that the second part of the problem, the sustaining of the machine in the air, would be realised.

The next day, Wednesday the 13th, no further trials were made on account of the rain and wind.

On Thursday the 14th the Chairman requested that General Grillon, who had just been appointed as a member of the Committee, accompany him so as to have a second witness.

The weather was fine, but a fairly strong, gusty wind was blowing from the south. Mr. Ader explained to the two members of the Committee the danger of these gusts since at two points of the circumference these would strike him sideways. The wind was blowing in the direction A. B., the apparatus starting from C. and running in the direction shown by the arrow. The first dangerous spot would be at B. The apparatus had been kept in readiness in the event of the wind dying down. Toward sunset the wind seemed to die down, as it had done on the evening of the 12th. Mr. Ader hesitated, which unfortunately further events only justified, but decided to make a new trial.

At the start which took place at 5.15 p.m. the apparatus having the wind in the rear seemed to run at a fairly regular speed, it was nevertheless easy to notice from the marks of the wheels on the ground that the rear part of the apparatus had been lifted and that the rear wheel, being the rudder, had not been in constant contact with the ground. When the machine came to the neighbourhood of B. the two members of the Committee saw the machine swerve suddenly out of the track in a semicircle, lean over to the right and finally stop. They immediately proceeded to the point where the accident had taken place and endeavoured to find an explanation for the same. The Chairman decided as follows:—

Mr. Ader was the victim of a gust of wind which he had feared as he explained before starting out; feeling himself thrown out of his course, he tried to use the rudder energetically, but at that time the rear wheel was not in contact with the ground and therefore did not perform its function; the canvas rudder, which had as its purpose the manœuvring of the machine in the air, did not have sufficient action on the ground. It would have been possible without any doubt to react by using the propellers at unequal speed, but Mr. Ader being still inexperienced had not thought of this. Furthermore, he was thrown out of his course so quickly that he decided in order to avoid a more serious accident to stop both engines. This sudden stop produced the half circle already described and the fall of the machine on its side.

The damage to the machine was serious; consisting at first sight of the rupture of both propellers, the rear left wheel and the bending of the left wing tip. It will only be possible to tell after the machine is taken apart whether the engines and more particularly the organs of transmission have been put out of line.

Whatever the damage may be, though comparatively easy to repair, it will take a certain amount of time, and taking into consideration the time of year it is evident that the tests will have to be adjourned for the present.

As has been said in the above report, the tests, though prematurely interrupted, have shown results of great importance, and though the final results are hard to foresee, it would seem advisable to continue the trials. By waiting for the return of spring there will be plenty of time to finish the tests and it will not be necessary to rush matters, which was a partial cause of the accident. The Chairman of the Committee personally has but one hope and that that a decision be reached accordingly.

Division General,

Chairman of the Committee,

Signed, MENSIER.

Boulogne-sur-Seine, October 21, 1897.

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ANNEX TO THE REPORT OF OCTOBER 21.

General Grillon, who was present at the trials of the 14th and who saw the report relative to what happened during that day, made the following observations in writing which are reproduced herewith in quotation marks. The Chairman of the Committee does not agree with General Grillon and he answers these observations paragraph by paragraph.

1.—" If the rear wheel (there is only one of these) left but intermittent tracks on the ground, does that prove that the machine has a tendency to rise when running at a certain speed? "

Answer.—This does not prove anything in any way and I was very careful not to mention this in my report, this point being exactly what was needed and that was not demonstrated during the two tests made on the grounds.

" Does not this unequal pressure of the two pair of wheels on the ground show that the centre of gravity of the apparatus is placed too far forward and that under the impulse of the propellers the machine has a tendency to tilt forward, due to the resistance of the air? "

Answer.—The tendency of the apparatus to rise from the rear when it was running with the wind seemed to be brought about by the effects of the winds on the huge wings, having a spread of 17 metres, and I believe that when the machine would have faced the wind the front wheels would have been lifted.

During the trials of October 12, when a complete circuit of the track was accomplished without incidents, as I and Lieut. Binet witnessed, there was practically no wind. I was therefore unable to verify whether during this circuit the two front wheels or the two rear wheels were in constant contact with the ground because when the trial was over it was dark (it was 5.30), and the next day it was impossible to see anything, because it had rained during the night and during Wednesday morning. But what would prove that the rear wheel was in contact with the ground at all times is the fact that Mr. Ader, though inexperienced, did not swerve from the circular track, which would prove that he steered pretty well with his rear wheel—this he could not have done if he had been in the air.

In the tests of the 12th, the speed was at least as great as on the 14th.

2.—" It would seem to me that if Mr. Ader thought that his rear wheels were off the ground he should have used his canvas rudder in order to regain his proper course; this was the best way of causing the machine to rotate, since it would have given an angular motion to the front axle."

Answer.—I state in my report that the canvas rudder whose object was the manœuvre of the apparatus in the air could have no effect on the apparatus on the ground, and to convince oneself of this point it is only necessary to consider the small surface of this canvas rudder compared with a mass to be handled on the ground, a weight of approximately 400 kg. According to my idea and as I have stated in my report, Mr. Ader should have steered by increasing the speed on one of his propellers and slowing down the other. He admitted afterwards that this remark was founded, but that he did not have time to think of it owing to the suddenness of the accident.

3.—" When the apparatus fell on its side it was under the sole influence of the wind, since Mr. Ader had stopped the machine. Haven't we a result here which will always be the same when the machine comes to the ground since the engines will have to be stopped or slowed down when coming to the ground? Here seems to be a bad defect of the apparatus under trial."

Answer.—I believe that the apparatus fell on its side after coming to a stop, not on account of the wind but because the semicircle described was on rough ground and one of the wheels had collapsed.

Signed, MENSIER.

October 27, 1897.

It will be seen that the report shows that the Ader machine never left the ground. *It started with the wind on its back and rolled along on the ground at a speed not greater than 12 or 14 miles an hour. The wind on its back and the pull of the screws tended to force the upper part of the apparatus forward, while the friction of the wheels on the ground retarded the machine below, so that*

there was a tendency of the apparatus to turn over on its nose and lift the rear wheel from the ground. A puff of wind rolled the machine over and smashed it to pieces.

The official report accords exactly with the statement made to us in 1906 by a member of the French Commission which came to America to negotiate with us in that year. He said that the machine never flew at all, but while running on the ground rolled over and was smashed. He also remarked that if the machine had really flown the Commission would not be in America negotiating with us. The failure of the Ader machine was the real cause of the failure of this negotiation with us, because the French officials at home could not believe that we had succeeded where Ader, with the assistance of the French Government, had failed, and they feared being laughed at if they closed a contract with us.

The Value of Ader's Works.

M. Ader occupies a different position in the history of attempts at flight from that of Lilienthal, Maxim, Langley and Chanute, by reason of the fact that his experiments were conducted in secret. Although Lilienthal, Maxim, Chanute, and Langley fell short of success in solving the flying problem, nevertheless their efforts contributed to the final success.

The Wright brothers had heard of Ader only as a man who was rumoured to have attempted to solve the problem of human flight by a slavish imitation of nature, and had met with disastrous failure. He deserved great credit for the persistence and energy with which he carried on experiments for a great number of years, but unfortunately he did not succeed himself in solving the problem nor in making his labours useful to others. Consequently his work contributed nothing to the final success.

Rumours have been circulated that the Ader machine was designed to control lateral balance by a method similar to that invented and given to the world by the Wright brothers. During the trial of the Wright French patent suit the Court visited the museum where the Ader machine is preserved and found that it contained no provision for varying the angles of the wings. Mr. Ader was present, and stated that the machine of 1890 possessed means for distorting the wings, but that the use of two propellers capable of being run at different speeds had made this distortion unnecessary in the 1897 machine. This statement proved that the abandoned device related to steering and not to lateral balancing.

Appendix G.

The following testimony made by Wilbur Wright, 15th February, 1912, describes in his own clear language how he and his brother Orville succeeded in producing the first flying machine to fly. This description is now published for the first time.

By Wilbur Wright.

My brother and I became seriously interested in the problem of flight in 1899, a little more than 12 years ago. Some three years before this the death of Lilienthal, which was mentioned in the newspapers of that day, had brought the subject to our attention and led us to make some inquiry for books relating to flight. But the only serious books we found were by Professor Marey, and these related to the mechanism of bird flight rather than human flight. As our interest at that time was mere curiosity as to what had been done, we did not pursue the subject further when we failed to find books relating to human flight.

Several years later, while reading a book on ornithology, we became interested in studying the appearance and habits of birds, but it soon occurred to us that the

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really interesting thing about birds was their power of flight. This was a power which seemed to us almost in contradiction of the laws of nature. But we saw them perform such wonderful feats, feats apparently many times more difficult than ordinary flight, and we could not help wondering why it was that man could not imitate at least the more simple manœuvres.

We knew that men had by common consent adopted human flight as the standard of impossibility. When a man said "it can't be done; a man might as well try to fly," he was understood as expressing the final limit of impossibility. Our own growing belief that men might nevertheless learn to fly was based on the idea that, while thousands of creatures of the most dissimilar bodily structures, such as insects, fishes, reptiles, birds, and mammals, were every day flying through the air at pleasure, it was reasonable to suppose that men also might fly. Of course, there might be, and doubtless would be, many serious difficulties to be overcome, but we thought that by learning what these difficulties were and finding methods of overcoming them, the problem of human flight might be solved, and we thought that probably the cheapest and best way to take up the subject would be to acquaint ourselves with the troubles which others had met in attempting to solve the problem.

We accordingly decided to write to the Smithsonian Institution and inquire for the best books relating to the subject. We had heard that the Smithsonian was interested in matters relating to human flight. In response to our inquiry, we received a reply recommending Langley's "Experiments in Aërodynamics," Chanute's "Progress in Flying Machines," and the "Aëronautical Annuals" of 1895, 1896, and 1897. These last were yearly publications, edited by James Means, giving from year to year reports of efforts being made to solve the flying problem. The Smithsonian also sent us a few pamphlets extracted from their annual reports, containing a reprint of Mouillard's "Empire of the Air," Langley's "Story of Experiments in Mechanical Flight," and a couple of papers by Lilienthal relating to "Experiments in Soaring."

When we came to examine these books we were astonished to learn what an immense amount of time and money had been expended in futile attempts to solve the problem of human flight. Contrary to our previous impression, we found that men of the very highest standing in the professions of science and invention had attempted the problem. Among them were such men as Leonardo da Vinci, the greatest universal genius the world has ever known; Sir George Cayley, one of the first men to suggest the idea of the explosion motor; Professor Langley, secretary and head of the Smithsonian Institution; Dr. Bell, inventor of the telephone; Sir Hiram Maxim, inventor of the automatic gun; Mr. O. Chanute, the past president of the American Society of Civil Engineers; Mr. Chas. Parsons, the inventor of the steam turbine; Mr. Thomas A. Edison, Herr Lilienthal, M. Ader, Mr. Phillips, and a host of others.

The period from 1889 to 1897 we found had been one of exceptional activity, during which Langley, Lilienthal, Chanute, Maxim, and Phillips had been feverishly at work, each hoping to win the honour of having solved the problem; but one by one they had been compelled to confess themselves beaten, and had discontinued their efforts. In studying their failures we found many points of interest to us.

At that time there was no flying art in the proper sense of the word, but only a flying problem. Thousands of men had thought about flying machines and a few had even built machines which they called flying machines, but these machines were guilty of almost everything except flying. Thousands of pages had been written on the so-called science of flying, but for the most part the ideas set forth, like the designs for machines, were mere speculations and probably 90 per cent. was false.

Consequently those who tried to study the science of aerodynamics knew not

what to believe and what not to believe. Things which seemed reasonable were very often found to be untrue, and things which seemed unreasonable were sometimes true. Under this condition of affairs students were accustomed to pay little attention to things that they had not personally tested.

From the writings of Langley, Lilienthal, Maxim, Chanute, etc., we obtained the best knowledge we could of the laws of aerodynamics, but as we went on we found that many things which we at first supposed to be true were really untrue; that other things were partly true and partly untrue; and that a few things were really true.

As to the state of experimental knowledge at the time we began our experiments, we reached the conclusion that the problem of constructing wings sufficiently strong to carry the weight of the machine itself, along with that of the motor and of the aviator, and also that of constructing sufficiently light motors were sufficiently worked out to present no serious difficulty; but that the problem of equilibrium had been the real stumbling block in all serious attempts to solve the problem of human flight, and that this problem of equilibrium in reality constituted the problem of flight itself.

We therefore decided to give our special attention to inventing means of retaining equilibrium, and as this was a field where mere speculation was of no value at all, we made a careful study of the state of the experimental knowledge. We found that prior to Lilienthal no one had made any serious attempt to leave the ground in a flying machine. All experiments in the air had resulted in such immediate disaster that the first trial was not usually followed up. But Lilienthal constructed several motorless apparatus, and with them began a study of the problem by actual experiments in the air. By this means he studied the carrying capacity of wings and investigated the various disturbances of equilibrium to which machines in the air are subjected, both as regards disturbances due to the direction and speed of the motion of the machine through the air and also as regards the disturbances produced by variations in the direction and speed of the wind itself.

The studies were continued for several years, but he met with a fatal accident and was killed before having found the solution.

His machine consisted of immovable supporting planes, somewhat similar in shape to the wings of large birds, and having at the rear a tail consisting of a vertical vane and a horizontal vane, mounted on a tail stick. Neither the vertical vane nor the horizontal vane could be operated by the aviator. Balance was to be effected by shifting the body of the operator toward that part of the aeroplane which attempted to rise, in the hope that the increased weight on that part would bring it down again. Such method was, from its nature, quite impracticable for application in practical flying, since in heavy motor-driven machines the weight which it would be practicable to move would be so small, in proportion to the total weight of the machine, that such movement would be quite inadequate to control equilibrium.

Even in the light glider he frequently found it insufficient, and several times was turned completely over in the air. In one such fall he was killed. His example, in adopting this method of experimentation, was followed by Mr. Chanute and his assistants and by Mr. Pilcher. After the death of Lilienthal, in 1896, Mr. Chanute discontinued his experiments, and, after a time, Mr. Pilcher fell and was killed.

The efforts of Mr. Maxim, Mr. Phillips, and M. Ader, the latter with the financial assistance of the French Government, to construct motor-driven aeroplanes had resulted in the abandonment of the experiments without flight having been attained. So that the period of unexampled activity, which extended from 1889 to 1897, was followed by one of complete collapse and despair, during which

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the attention of the world was turned entirely to dirigible balloons, which, at this time, were being brought into prominence by Santos Dumont.

During the "boom" period fully a half-million dollars had been expended under the direction of some of the ablest men in the world and two lives had been lost. When we studied the story of loss of life, financial disaster, and final failure which had accompanied all attempts to solve this problem of human flight, we understood more clearly than before the immensity and the difficulty of the problem which we had taken up. But as we studied the story of their troubles and considered how and why they failed, we could not help thinking that many of their troubles might have been avoided and that others might have been overcome by the adoption of more adequate methods.

We began to study the flight of birds to see whether they really used the methods of maintaining equilibrium which Chanute and Mouillard had represented the birds as using. They had represented that the birds maintained fore and aft balance by moving the wings forward and backward so as to bring the centre of pressure of the wings to the front or to the rear of the centre of gravity, and thus tilt the bird upward in front or upward at the rear, as occasion required. They represented that lateral balance was maintained by drawing inward one wing so as to reduce its area as compared with the wing on the other side, so as to reduce the lift on the side which tended to rise. They also said that the bird sometimes rocked its body over toward the high side in order that the increase of weight on that side might help to bring the high wing down.

But in watching the flight of some pigeons one day we noticed one of the birds oscillate rapidly from side to side, that is, it tilted so that one wing was elevated above its normal position and the other depressed below its normal position and then tilted in the opposite direction. These lateral tiltings, first one way and then the other, were repeated four or five times very rapidly; so rapidly, in fact, as to indicate that some other force than gravity was at work. The method of drawing in one wing or the other, as described by Chanute and Mouillard, was, of course, dependent in principle on the action of gravity, but it seemed certain that these alternate tiltings of the pigeon were more rapid than gravity could cause, especially in view of the fact that we could not detect any drawing in first of one wing and then of the other.

In considering possible explanations of the method used by the bird in this instance, the thought came that possibly it had adjusted the tips of its wings about a lateral transverse axis so as to present one tip at a positive angle and the other at a negative angle, thus, for the moment, turning itself into an animated windmill, and that when its body had revolved on a longitudinal axis as far as it wished, it reversed the process and started to turn the other way. Thus the balance was controlled by utilising dynamic reactions of the air instead of shifting weight. So far as fore and aft balance is concerned, this seemed to be accounted for by fore and aft movements of the wings, as claimed by Chanute.

In speculating on possible methods of constructing a flying machine to carry a man, we hit on the idea of providing a structure consisting of superposed surfaces rigidly trussed along their front and rear margins, somewhat after the general style of the Chanute "double-decker," but not trussed from front to rear. The connections of the uprights joining the two surfaces were to be hinged so that the upper surface could be moved forward or backward, with reference to the lower surface. This would have an effect on fore and aft balance similar to that produced by the fore and aft movement of the wings of birds. I refer, of course, to the slight fore and aft movements of the wings of soaring birds, like the buzzards and hawks, made for the purpose of balancing. It is an entirely distinct thing from up and down flapping. It was designed to move either end of the upper surface forward or backward by a separate lever, one controlling one tip and the other the other. If both levers were pressed forward the upper surface would be moved bodily forward and the machine would turn upward, but if one

lever were thrown forward and the other backward, one tip of the upper surface would move forward and the other backward. Thus there would be no change in the general position of the upper surface to the front or rear of its normal position, but the entire structure, consisting of both the upper and lower surface, would be given a warp like that shown in our patent. We reasoned that by imparting such warp we could control lateral balance of the machine, either for the purpose of balancing or steering, as we had noticed that when the birds were tilted they circled around the depressed wing.

In this design it was not intended to use either vertical or horizontal vanes or rudders of any kind. We reasoned that all the evolutions of flight could be attained by the various combinations of movements of the two levers controlling the two ends of the upper surface. This speculation was very interesting from a theoretical standpoint, but when we came to consider it from the standpoint of practical invention we were convinced that without any supplementary horizontal surface the machine would be too erratic to be controlled by an aviator, and, besides, that it would call for an exertion of strength much beyond that possessed by a human being, both during flight and in landing.

Before attempting to construct a glider on this general principle we worked out the construction of the supporting planes and the mode of flexing a horizontal rudder. The horizontal rudder was placed at the front. There was no tail of any kind, either vertical or horizontal. With machines of this description we made experiments in the years 1900 and 1901 on the seashore near Kitty Hawk, N.C. It was our idea that the method of experimentation of gliding had been so discredited by the deaths of Lilienthal and Pilcher that we intended to practice with this apparatus by attaching it to a short horizontal rope and letting it float in a strong wind a few feet from the ground, while we practised the manipulation of the horizontal front rudder and the warping of the wings to maintain the apparatus in balance. But we found that a stronger wind than the scientific calculations of other experimenters indicated was necessary in order to sustain this machine.

It was, therefore, necessary to resort to gliding in order to attain a relative wind strong enough to sustain this apparatus. We experimented first with the warping wires fastened tight and used the front rudder only. We feared that if we attempted to control both we would not properly control either, as we were without any training. We therefore glided down a slope controlling our up and down movement and balance by adjustments of the horizontal front rudder. If the machine attempted to turn over sidewise we brought it to the ground. The flights were made at first at a height only of one or two feet.

We found that the flexible front rudder was very efficient in controlling the fore and aft balance. We also found that frequently we could make glides of 15 to 20 seconds without being tilted laterally sufficiently to necessitate landing. If the tilting was bad we immediately brought the machine down.

After we had acquired some skill in handling the horizontal front rudder we loosened the warping wires and attempted to control the lateral balance also, but when we did this we found ourselves completely nonplussed. The apparatus did not act at all as we had expected. At first we were not able to determine exactly what it did do, but it was clear enough that it was not what we wanted in all respects. We repeated the trials for the purpose of determining, if possible, exactly what happened, but found this no easy task.

To the person who had never attempted to control an uncontrollable flying machine in the air this may seem somewhat strange, but the operator on the machine is so busy manipulating his rudder and looking for a soft place to alight that his ideas of what actually happens are very hazy. It is much nicer to sit before a pleasant fire and speculate than to work out, at the risk of life and limb, the constructions necessary to reduce speculation to practical invention.

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We repeated this experiment time and again, and several times barely escaped disaster. We found that if we jerked the warping cradle back and forth rapidly the machine would make its way down the hill, but if we persisted in the movement long enough to determine its real effect the machine quickly acquired such a peculiar feeling of instability that we were compelled to instantly seek the ground.

After repeated experiments we began to perceive that in landing the machine was skidding somewhat toward the wing having the smaller angle and was facing somewhat toward the wing having the greater angle, and the wing having the greater angle seemed to touch first. As our season was now at a close, we were compelled to leave the problem in this condition.

These experiments constituted the first instance in the history of the world that wings adjustable to different angles of incidence on the right and left sides had been used in attempting to control the balance of an aeroplane. We had functionally used them both when flying at the end of a rope and also in free flight.

When we left Kitty Hawk at the end of 1901 we doubted that we would ever resume our experiments. Although we had broken the record for distance in gliding, so far as any actual figures had been published, and although Mr. Chanute, who was present part of the time, assured us that our results were better than had ever before been attained, yet when we looked at the time and money which we had expended, and considered the progress made and the distance yet to go, we considered our experiments a failure. At this time I made the prediction that men would some time fly, but that it would not be within our lifetime. In view of our own experience and in view of the experience of men like Langley, Lilienthal, Maxim, Chanute, and Ader, men almost ideally fitted in mental equipment and training for such work, and having at their command hundreds of thousands of dollars, all of whom, like ourselves, had found the results attained too small for the effort and money expended, and who had, one by one, abandoned the task before we had taken it up, we felt that similar conditions would probably prevail for a long time, as the problem of stability, which had caused all these men to drop the problem, was yet seemingly untouched, so far as the practical solution was concerned.

After our return home we could not keep our minds off of the puzzling things we had observed, nor keep from studying possible solutions of our difficulties, and before long we were as deeply interested as before. In studying our troubles relating to lateral balance, we reasoned that possibly the trouble might be due to the fact that the wing to which an increased angle of incidence had been imparted would receive not only an increased lift, but also an increased backward pressure or resistance, and that this might so decrease the speed of that wing, as compared to the opposite wing, that its lift would be reduced sufficiently from this cause to wipe out the increase in lift, due to its greater angle of incidence. It is a well-known law of aerodynamics that the lifting pressure varies as the square of the speed at which the aeroplane and wind strike each other, so that if the wing of the greater angle lagged behind while the other wing gradually forged ahead, one wing would be moving at a different speed from the other, and by reason of this speed would have a different lift, the slower wing, of course, having the lesser lift. We reasoned that if the speeds of the right and left wings could be controlled the advantage of the increased angle of incidence of one wing and decreased angle of the other could be utilised as we had originally intended.

Two ways of controlling the relative speeds of the wing tips were open to us; one consisting in providing means for creating variable resistances at the wing tips at the will of the operator, so that the wing which tends to forge ahead could be retarded; the other consisted in providing a surface at the rear with which a torque about a vertical axis could be created, to counterbalance that produced by the difference in resistance of the wing tips.

We decided to use the surface at the rear, on account of its greater dynamic efficiency, since every pound of resistance at the wing tips would cost an extra pound of push in the propeller, while with the surface at the rear, exposed almost edgewise, eight or ten pounds of turning power could be obtained at an expenditure of one pound backward resistance or one pound of propeller thrust. And, for the sake of simplicity, we decided to use a fixed vertical vane, as we reasoned that if the machine attempted to turn on a vertical axis the vane at the rear would be exposed more and more to the wind and would stop further turning of the machine as soon as the vane was exposed enough to receive a turning pressure equal to that produced in the opposite direction by the difference in the resistances of the wing tips when adjusted to different angles of incidence. Thus the vane would be exposed to the wind on the side toward the wing having the smaller angle of incidence.

In the fall of 1902 we returned to Kitty Hawk with an apparatus fitted with a fixed vertical vane at the rear. When we tried the apparatus we found that under favourable conditions the apparatus performed as we had expected, so that we could control lateral balance or steer to the right or left by the manipulation of the wing tips.

This was the first time in the history of the world that lateral balance had been achieved by adjusting wing tips to respectively different angles of incidence on the right and left sides. It was also the first time that a vertical vane had been used in combination with wing tips, adjustable to respectively different angles of incidence, in balancing and steering an aeroplane.

But as we proceeded with our experiments we found that the expected results were not always attained. Sometimes the machine would turn up sidewise and come sliding to the ground in spite of all the warp that could be imparted to the wing tips. This seemed very strange. The apparatus would sometimes perform perfectly, and at other times, without any apparent reason, would not perform at all. Every now and then it would come tumbling to the ground and make such a rough landing that we often considered ourselves lucky to escape unhurt.

By taking the chance over and over we finally began to notice the conditions under which the difficulty was liable to occur. It seemed that when the machine was tilted laterally it began to slide sidewise while advancing, in accordance with the well-known law of gravitation, just as a sledge slides downhill or a ball rolls down an inclined plane, the speed increasing in an accelerated ratio. If the tilt happened to be a little worse than usual, or if the operator was a little slow in getting the balance corrected, the machine slid sidewise so fast that the sidewise movement of the machine caused the vertical vane to strike the wind on the side toward the long wing, instead of on the side toward the high wing, as it should have done.

In this state of affairs the vertical vane, instead of counteracting the turning of the machine about a vertical axis, as the result of the difference of resistance of the warped wings on the right and left sides, on the contrary assisted in its turning movement, and the result was worse than when the vertical vane was absent. We felt that if this was the true explanation it would be necessary to make the vertical vane movable, in order that the pressure on the side toward the low wing might be relieved and the pressure brought to bear on the side toward the high wing. We spent several days in experimenting to make sure that this was the real cause of the difficulty.

Meanwhile my brother, in thinking about the matter, noted that a particular relation existed in the desired pressures on the rudder, no matter whether the troubles were due to differences of resistances of the wing tips or whether they were due to sliding. In either case it was desirable to get rid of the pressure on the side toward the low wing, to which a greater angle of incidence must be imparted, in restoring lateral balance, and brought to bear on the side of the vertical

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tail which is toward the high wing to which the reduced angle of incidence must be imparted in such case. For the sake of simplicity we therefore decided to attach the wires controlling the vertical tail to the wires warping the wings, so that the operator, instead of having to control three things at once, would have to attend to only the forward horizontal rudder and the wing warping mechanism; and only the latter, alone, would be needed for controlling lateral balance.

We now had the structure in the form pictured and described in the drawings and specification of our patent. With this apparatus we made nearly 700 glides in the two or three weeks following. We flew it in calms and we flew it in winds as high as 35 miles an hour. We steered it to right or left, and performed all the evolutions necessary for flight.

This was the first time in the history of the world that a movable vertical tail had been used in controlling the direction or the balance of a flying machine. It was also the first time that a movable vertical tail had been used, in combination with wings adjustable to different angles of incidence, in controlling the balance and direction of an aeroplane. We were the first to functionally employ a movable vertical tail in a flying aeroplane. We were the first to employ wings adjustable to respectively different angles of incidence in a flying aeroplane. We were the first to use the two in combination in a flying aeroplane.

We built a second machine and flew it in a field near the City of Dayton, Ohio, in the summer and autumn of 1904. When we had familiarised ourselves with the operation of the machine in more or less straight flights we decided to try a complete circle. At first we did not know just how much movement to give in order to make a circle of a given size. On the first three trials we found that we had started a circle on too large a radius to keep within the boundaries of the small field in which we were operating. Accordingly, a landing was made each time, without accident, merely to avoid passing beyond the boundaries of the field.

On the fourth trial, made on September 20, a complete circle was made, and the machine was brought safely to rest after having passed the starting point. Thereafter we repeatedly made circles, and on November 9 made four circles of the field in a flight lasting a few seconds over five minutes.

In all these flights the warping wires and the wires controlling the vertical tail were interconnected, as in our patent. In order to circle to the left, we moved the cradle slightly to the left, thus turning the tail slightly to the left and imparting an increased angle to the right wing and a smaller angle to the left wing. This caused the machine to tilt so that the left wing was lower than the right wing, which, of course, in turn, caused the machine to slide somewhat to the left. This side movement of the machine tended to cause the vertical rudder to strike the air at a greater angle than was necessary to compensate for the difference in resistance of the right and left wings. This tendency caused the tail to lag behind in this lateral movement, just as the feather of an arrow causes the feathered end to lag behind when the arrow is dropped sidewise. Thus the lateral movement of the main aeroplane sidewise, as the result of tipping, became combined with the rotary movement about its vertical axis, due to the vane-like action of the tail, and the machine proceeded on a circular course.

But as the speed of the outside wing increased, and that of the inside wing decreased, by reason of the fact that the inner wing was travelling in a smaller circle than the outside wing, there was a tendency to tilt too much, and this was corrected by gradually moving the cradle toward the high wing, thus increasing the angle on the low wing and decreasing the angle of the high wing, and also setting the rudder over toward the high wing. This was done gradually, but only sufficiently to prevent the low wing from sinking lower and not enough to bring it back to the level. The machine then continued to circle to the left, with the vertical tail set over somewhat to the right, so that the machine turned in the

opposite direction to that in which a ship would have turned with the ship's rudder set over to the right.

When it was desired to stop circling a sudden movement of the cradle toward the high side gave the wings an increased warp and brought the machine up to the level. Then on setting the cradle back to its central position, thus restoring the wings and tail to their central positions, the machine proceeded in a straight line, with the wings level.

With this machine we made approximately 100 flights in the year 1904. Usually the machine responded promptly when we applied the control for restoring lateral balance, but on a few occasions the machine did not respond promptly, and the machine came to the ground in a somewhat tilted position. The cause of the difficulty proved to be very obscure, and the season of 1904 closed without any solution of the puzzle.

In 1905 we built another machine and resumed our experiments in the same field near Dayton, Ohio. Our particular object was to clear up the mystery which we had encountered on a few occasions during the preceding year. During all the flights we had made up to this time we had kept close to the ground, usually within 10 feet of the ground, in order that in case we met any new and mysterious phenomenon we could make a safe landing.

With only one life to spend we did not consider it advisable to attempt to explore mysteries at such great height from the ground that a fall would put an end to our investigations and leave the mystery unsolved.

The machine had reached the ground, in the peculiar cases I have mentioned, too soon for us to determine whether the trouble was due to slowness of the correction or whether it was due to a change of conditions, which would have increased in intensity, if it had continued, until the machine would have been entirely overturned and quite beyond the control of the operator. Consequently it was necessary, or at least advisable, to discover the exact cause of the phenomenon before attempting any high flights. For a long time we were unable to determine the peculiar conditions under which this trouble was to be expected. But as time passed we began to note that it usually occurred when we were turning a rather short circle.

We therefore made short circles sometimes for the purpose of investigating and noting the exact conduct of the machine from the time the trouble began until the landing was made. At one time we thought it might be due to some special reaction of the air, due to the fact that the machine, in circling, did not face exactly in the direction of the line of motion. To test this point we disconnected the rudder wire from the warping wire and operated the rudder by an entirely separate handle. The trouble, however, continued as before. A flight has been referred to in the "Century Magazine" article of September, 1908, in the following language:—

> "We had not been flying long in 1904 before we found that the problem of equilibrium had not as yet been entirely solved. Sometimes, in making a circle, the machine would turn over sidewise despite anything the operator could do, although, under the same conditions in ordinary straight flight, it could have been righted in an instant. In one flight, in 1905, while circling around a honey locust-tree at a height of about 50 feet, the machine suddenly began to turn up on one wing, and took a course toward the tree. The operator, not relishing the idea of landing in a thorn-tree, attempted to reach the ground. The left wing, however, struck the tree at a height of 10 or 12 feet from the ground, and carried away several branches; but the flight, which had already covered a distance of six miles, was continued to the starting point."

The flight here mentioned was made on September 28, 1905, with the rudder wires entirely disconnected from the warping wires. When it was noticed that the machine was tilting up and sliding toward the tree the operator turned the

machine down in front and found that the apparatus then responded promptly to the lateral control. The remedy was found to consist in the more skilful operation of the machine, and not in a different construction.

The trouble was really due to the fact that in circling the machine has to carry the load resulting from centrifugal force, in addition to its own weight, since the actual pressure that the air must sustain is that due to the resultant of the two forces. The machine in question had but a slight surplus of power above what was required for straight flight, and as the additional load, caused by circling, increased rapidly as the circle became smaller, a limit was finally reached beyond which the machine was no longer able to maintain sufficient speed to sustain itself in the air. And as the lifting effect of the inner wing, owing to its reduced speed, counterbalanced a large part of the increased lift resulting from the greater angle of incidence on that wing, the response to lateral control was so slow that the machine sank to the ground usually before it had been brought back to the level again. In other words, the machine was in what has come to be known as a " stalled " condition.

The phenomenon is common to all the aeroplanes in the world and is the cause of frequent disaster to unskilled aviators. Our own machine is still subject to the same trouble. Within the last year four or five Wright machines have been wrecked by novices stalling the machines in attempting to climb too fast while circling, and have come tumbling to the ground, just as we did in 1905. Similar wrecks of other makes of machines now occur almost every week at the foreign flying schools. The remedy for the difficulty lies in more skilful operation of the aeroplanes.

When we had discovered the real nature of the trouble and knew that it could always be remedied by tilting the machine forward a little, so that its flying speed would be restored, we felt that we were ready to place flying machines on the market. We spent the next two years in building machines and making business arrangements for the exploitation of the patent. In 1908 we sold a machine to the United States Government, and in the years 1908 and 1909 flights were made before the officials of the United States, at Washington, and before the rulers of England, France, Spain, Italy, and Germany.

APPENDIX H.

From the AËRONAUTICAL JOURNAL, *Vol. X., January*, 1906.

GENERAL MEETING.

The opening meeting of the Forty-First Session of the Aëronautical Society of Great Britain was held at the Society of Arts, John Street, Adelphi, on Friday, December 15th (1905). The President, Major B. F. S. Baden-Powell, was in the chair.

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RECENT EXPERIMENTS OF THE BROTHERS WRIGHT.

Mr. Alexander then read the following letter from the Wright Brothers, dated November 17, 1905 :—

" We have finished our experiments for this year after a season of gratifying success. Our field of experiment, which is situated eight miles east of Dayton, has been very unfavourable for experiment a great part of the time, owing to the nature of the soil, and the frequent rains of the past summer. Up to September 6 we had the machine on but eight different days, testing a number of changes which we had made since 1904, and as a result the flights on these days were not so long

as our ones of last year. During the month of September we gradually improved in our practice, and on the 26th made a flight of a little over 11 miles. On the 30th we increased this to twelve and one-fifth miles, on October 3 to fifteen and one-third miles, on October 4 to twenty and three-fourth miles, and on the 5th to twenty-four and one-fourth miles. All of these flights were made at about thirty-eight miles an hour, the flight of the 5th occupying thirty minutes three seconds. Landings were caused by the exhaustion of the supply of fuel in the flights of September 26 and 30, and October 8, and in those of October 3 and 4 by the heating of bearings in the transmission of which oil cups had never been fitted. But before the flight on October 5 oil cups had been fitted to all the bearings, and the small gasolene can had been replaced with one that carried enough fuel for an hour's flight. Unfortunately, we neglected to refill the reservoir just before starting, and as a result the flight was limited to 38 minutes. We had intended to place the record above the hour, but the attention these flights were beginning to attract compelled us to suddenly discontinue our experiments in order to prevent the construction of the machine from becoming public.

"The machine passed through all of these flights without the slightest damage. In each of these flights we returned frequently to the starting point, passing high over the heads of the spectators.

"ORVILLE WRIGHT."

THE PRESIDENT: I think that sounds like a remarkable statement. We have not heard much of what the Brothers Wright have been doing recently. We heard a year or two ago that they had made some successful flights, but this sounds a very successful result. I shall certainly be longing to hear more of the details of these flights. To remain half an hour in the air seems extraordinary.

APPENDIX I.

From the AËRONAUTICAL JOURNAL, *April*, 1906, *Vol. X.*

THE EXPERIMENTS OF THE BROTHERS WRIGHT.

As is notified in another part of this Journal, no less an authority than Sir Hiram Maxim has promised to address the members of the Aëronautical Society of Great Britain, on the subject of the recent experiments of the Brothers Wright, at the meeting of the Society on April 27th next. Many remarks on these experiments will therefore be now deferred. It may, however, be here stated that various reports which have come to hand seem to confirm the statements made by the Brothers Wright in the letter which was read to the members of the Aëronautical Society of Great Britain, at the opening meeting of the present session.

Our contemporary, *L'Aerophile*, in its January issue, has published many interesting statements obtained from various sources, concerning these remarkable experiments which may be the commencement of a new era in aerial navigation. The following details derived from *L'Aerophile*, and various other sources, may be of interest to our readers, though in the absence of the precise information which is still withheld by the experimenters, no responsibility for the accuracy of these details can be taken. Conflicting statements as to some of these details must also be regarded as a sign that full confirmation must be awaited before assigning them to the history of aerial navigation. As far as can be gathered from accounts received, the frame of the machine which in form is evidently very similar to the older double-decked gliding machines of Messrs. Wright, and familiar to our readers, is made of larch wood. From tip to tip of the wings, its length is 40 feet, its breadth in the widest part is only six feet. The frame is covered with canvas. The gasoline motor, is stated in some accounts, to be 15

horse-power, in others, 21 horse-power. The total weight of the machine is 925 lbs., that of the motor alone, 40 lbs. The operator lies face downwards, in a horizontal position on the lower plane, and as close to the frame as possible. Since a weight hanging below the centre of gravity, like a pendulum, causes the aeroplane to capsize, all weight has to be concentrated as near as possible in a common plane. As to the steering apparatus, there is some discrepancy in the accounts. Some describe the rudder, which controls the vertical movements, as being in the middle of the machine, and the one which controls the horizontal movements as being at the rear.

It appears that the machine goes out of its shed on an inclined platform, mounted on an ingeniously constructed carriage, it glides on a rail to the end of the platform, when the motor is started, and the machine takes immediate flight.

APPENDIX J.

From the AËRONAUTICAL JOURNAL, *July*, 1906, *Vol. X.*

GENERAL MEETING.

The second meeting of the Forty-First Session of the Aëronautical Society of Great Britain was held at the Society of Arts, on Friday, April 27th (1906). The President, Major B. Baden-Powell, was in the chair.

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THE RECENT EXPERIMENTS CONDUCTED BY THE WRIGHT BROTHERS.

BY SIR HIRAM S. MAXIM.

I think we should all congratulate ourselves on the great success of the experiments conducted in the United States by the Wright Brothers. These experiments have demonstrated in a most conclusive manner that it is not only possible to navigate the air, but that the only chance of success is with machines heavier than the air. It can no longer be said that flying machines are impossible; a flying machine has at last been made which actually rises and travels through the air at a speed of about forty miles an hour without the aid of a gas bag. This machine marks a distinctly new epoch in aeronautical science. The first flying machine has come, and, whether we like it or not, it has come to stay. No less a genius than Edison himself has pointed out its potency as an instrument of warfare. It is impossible to over-estimate the changes that will take place during the next ten years in everything relating to civilised warfare. The flying machine must become a very important factor, and it behoves all the civilised nations of the earth to lose no time in becoming acquainted with this new means of attack and defence. The nations which do not appreciate the importance of this instrument of destruction will be very soon left in the lurch. The French have always been the most progressive nation on earth in everything relating to warfare, and the French Government was the first to expend large sums of money with a view of navigating the air. No sooner had the Wright Brothers made a success of their machine than it was eagerly sought after by the French Government, and I am of the opinion, notwithstanding that the first decided success has been met with in the United States, that in the immediate future the French will lead the van in everything relating to the subject of artificial flight.

Experiments in flying machines are enormously expensive, as a rule too expensive to be conducted by a private individual, but in France the Government is only too ready to furnish the necessary money, and now that the Americans have shown the world how to do it, it will not be long before our friends across the Channel will be supplying their army with veritable flying machines. If one

had taken up the subject thirty-five years ago it would not have been as easy as now. Strong aluminium and strong and thin steel tubes were not available at that time, but these are of small importance compared with the motor. During the last dozen years perhaps a million pounds sterling has been spent in the various countries of the world on experimental work relating to internal combustion engines, and always in the direction of great lightness in proportion to the power developed. We have to thank our friends the automobile makers for this advancement. It is now possible for the experimenter on flying machines to practically find the motor ready made, that is, the motor is ready at hand which will develop the power of a horse with the weight of a common barnyard fowl. It is very gratifying to me to know that the machine built by the Wright Brothers has been built on the same lines that I experimented on years ago; like my machine it has superposed aeroplanes propelled through the air by a screw. At the time of making my experiments the high power petroleum engine had not been invented; I was forced to use steam. My machine was too large and I did not have sufficient room to conduct my experiments; however I did prove the lifting effect of aeroplanes. I found that it was possible even with a steam engine to make a machine that would lift more than its own weight with three men on board. In the autumn of 1904 I conducted some experiments at the Crystal Palace with very sharp and thin aeroplanes, very slightly concaved on the under side and placed at a slight angle above the horizontal. These aeroplanes were driven through the air at an enormous velocity around a circle about 300 ft. in diameter, when it was found that the lifting effect was much greater than anyone had ever supposed. The lifting effect of aeroplanes is very much influenced by the motion of the air through which they are driven. In my Baldwin's Park experiments it was found that with a wind of no greater velocity than four miles an hour blowing across the track the lifting effect on the windward side would be no less than say 1,000 lbs. greater than on the lee side. It was originally intended by me to provide all the boats on my captive flying machines with aeroplanes, and to use about 200 h.p. for driving the ten boats; aeroplanes were placed on the first boats, but one day, when the wind was blowing forty miles an hour, one of the boats was lifted to a great height when travelling against the wind only to fall very quickly when it was travelling with the wind. The engineers of the L.C.C. were present, and I think somewhat alarmed at what they saw; it was only too evident that they would not pass such a machine. The aeroplanes were too dangerous to use except in a dead calm; they were taken off, and these machines ceased to be captive flying machines and degenerated into roundabouts in everything but name. I am, however, of the opinion that although the aeroplane system is the first to meet with success, still it is not the only system, and before many years I think we shall have machines made on a system which will be more manageable, less dangerous, and which will approximate nearer the flight of birds; in any case, the Wright Brothers deserve our warmest praise and congratulations.

The PRESIDENT: Has any gentleman any remarks to make on this subject?

Mr. NICHOLSON: I should like to be allowed to say that I think we are all indebted to Sir Hiram Maxim for the wonderful work he has done, and we should, I think, tender to him a most hearty vote of thanks for the trouble and expense he has been put to in the interests of this delightful science. (Applause.)

Mr. CODY: I can only second our friend's opinion of Sir Hiram's work. I have never had the pleasure of seeing his flying machines, but I agree with his idea as to aeroplanes lifting as they do. I do disagree with almost all science as regards the spread of a flying machine. A good machine will not fly and lift more than half a pound in a square foot. The Brothers Wright claim to have flown with 900 pounds, and 600 square feet. I doubt whether the Wright Brothers lifted and flew with that weight. I shall try to make a machine weighing 1,000 pounds, with 3,000 square feet of lifting surface, and capable of discharging lumps of lead or dynamite, or anything else to the extent of 500 pounds, that is

to say, when the machine is fully loaded, it will carry 1,500 pounds, and be capable of flying with 500 pounds less.

Col. FULLERTON: I should like to ask the speaker what the actual surface of the Wrights' machine is. I did not quite catch what he said.

Mr. CODY: I have been told by Mr. Alexander that it weighed about 900 lbs., had about 600 square feet of lifting surface, and had a 13 or 14 h.p. engine on it.

Mr. MOORE-BRABAZON: The Wright Brothers were the first who had this aeroplane, and they worked it by altering the weight. They had no arrangement for altering the equilibrium.

The CHAIRMAN: In this matter we all feel very much the uncertainty in which we are at present placed as to the details of the Wrights' machine. It seems to be very well authenticated that they have flown through the air and remained up for a considerable time. There is an interesting letter from Mr. Octave Chanute which has been reproduced in the "Automotor Journal," and I have had a letter myself from Mr. Octave Chanute in which he does not give many details, but he describes having seen some of these ascents, and we know how truthful he has been in his accounts of the kind. Mr. Chanute tells me that the Wright Brothers are constructing a second machine which will doubtless be a great improvement on their first.

There is one point Sir Hiram mentioned, and that is about light engines, light motor engines. Some light engines have been produced in France by the Boucher Co., and there is one I shall hope to get shortly which gives 12 h.p. and weighs only 52 lbs. They have made some of their engines of a lighter weight, under 3 lbs. per h.p. This is lighter than we ever heard of a few years ago. A light engine is a very great step towards the practical solution of a flying machine. One of the points Mr. Chanute has pointed out in his letter as regards the Wright Brothers' machine is that these gentlemen have not only created a record in inventing a flying machine, but also in being able to build a machine and experiment with it in the open air without the accounts of it getting into the papers. In America, more than in England, it is difficult to keep things out of the public press.

I will ask you to record your vote of thanks to Sir Hiram Maxim for his interesting paper.

The vote was carried unanimously.

APPENDIX K.

From the AËRONAUTICAL JOURNAL, *July*, 1912, *Vol. XVI.*

WILBUR WRIGHT.

Gold Medallist of the Society.

BY GRIFFITH BREWER, A.F.AË.S.

The present generation will never completely realise the magnitude of the loss to Aviation caused by the death of Wilbur Wright, and yet it requires but a moment's thought to recognise that his name will live until all histories are obliterated, for with his brother Orville, he accomplished the dream of ages, which men of acknowledged science had become in the habit of first attempting to accomplish, and then alleging the feat to be amongst the unattainable.

There are only three ways of getting from one place to another on this earth; one is on land, the next is on water, and the third is now through the air. Certainly it was possible to travel in the air to destinations settled by the caprice of the wind; but to do the same as the birds, and travel from one place to another, had never been possible until Wilbur and Orville Wright accomplished mechanical

flight in 1903. So great was this achievement, that when Wilbur Wright came to France to show how this thing could be done, the doubters came from all over the world to see with their own eyes the achievement of the impossible.

In August, 1908, I went to Le Mans to see with my own eyes what I, like others, had hitherto doubted, and when the flying was over for the day and the machine had been put back into its shed, Mr. Wright invited me to dine with him at the little Inn, where Madam Pollet served a plain, well-cooked meal in the little sanded kitchen.

I found Wilbur Wright quite open about his work; there was no air of mystery or cloaking the achievement of flight with artificial embellishments. Just the simple story of two brothers who had always played together and worked together, and one whose health required constant out-door exercise. At first they made a glider, which not being built upon books, had none of the difficulties enumerated in them, but had others not previously hinted at; and he told how in their holidays which grew longer as their work time grew less, they gained more experience in the art of gliding until they decided on the details of the power-driven machine. And as he told me of the fun they had had at Kitty Hawk with the hot wind blowing on the little tin shed and the thrills enjoyed in the first flights, I realised what lucky brothers they were, to have been blessed with the necessary intelligence, perseverance and love of roughing it, to make such glorious holidays brimful of recreation and enjoyment and yet so useful to mankind. Few know how the experiments were nearly abandoned on some occasions, and how their rival flyers the mosquitoes combined together in their millions to prevent the invasion of their element by man. How many resolutions to pack up and go home to-morrow were made in the night when those tiny insects, which in their myriads blackened the walls of the shed, succeeded in entering the blankets in which the brothers rolled themselves in spite of a night temperature of 90°. Fortunately the tortures of the night were forgotten in the enthusiasm of the day, and so they persevered and robbed the birds of their monopoly.

Many of my early meetings with Wilbur Wright were associated with the late Hon. C. S. Rolls. It was in August, 1908, when Rolls found other engagements prevented his taking part in the Gordon Bennett Balloon Race from Berlin, that he asked me to dine with him and talk over the details of my racing in his stead. He had just returned from Paris, and I was leaving for Paris that same evening, and after we had talked balloons and arranged for Mr. McClean to act as my aid in the race, he turned to me and said, "Why are you going to Paris?" In reply, I said: "Don't tell anyone, but I'm going on to Le Mans to see Wilbur Wright." "Well, don't say anything," Rolls replied, "but I've just come back from there." He then gave me some local directions about how to get to the Champ d'Auvours and where to stay at Le Mans. Afterwards I always stayed at the little Inn near the railway, because then I could be out early if the weather were fine, and in this way I saw many flights I should otherwise have missed.

The simple life had great attractions for Wilbur Wright. A piece of stout canvas nailed between two pieces of 2in. by 3in. wood supported at their ends on the rafters of the shed containing the machine, formed the bed on which he slept, and often at five in the sparkling September mornings he was to be seen taking an early cold water wash while the water for the coffee was on the boil. The machine was always his first care, for it was the only machine available and it served for all the flights of those last four eventful months of the year 1908.

The same shed, which by the by was too small to take the machine until the rudder had been folded back, served as his office as well as his bedroom, and as may be well imagined, the popular interest in those early flights attracted a large and in some cases unwelcome correspondence. He had no secretary to look after his letters, and most of his time being taken up with the machine, flying, and answering questions, correspondence could only have very limited attention. The most important letters were attended to at the time, and the remainder were placed

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in the pile to be answered when time permitted. Many persons who wrote did not seem to realise that Wilbur was one of the hardest worked men, who could not spare time to gratify the idle curiosity of the thousands who were naturally interested.

Some weeks later, just before the Gordon Bennett Race in Berlin, I came again to Le Mans in the hope of having a flight. Mr. Rolls, Mr. Butler and Major Baden-Powell were also there, all with the same hope in their minds. We were so infected with the flying fever, that we were tumbling over each other in the hope of getting the first invitation, and this hope was perhaps intensified by the knowledge that up to that time no Englishman had yet flown. Poor Rolls was torn by two anxieties, one to be given the first flight, and the other to get me off to Berlin so that I should not be too late to compete as his substitute in the Gordon Bennett Race, and the latter fear predominated to such an extent, that he most unselfishly suggested to Wilbur that I should go up first. Whether this turned the balance in my favour or not I cannot say, but after making other trial flights Wilbur came to me and asked if I was ready, so I took my place between him and the engine, and we were launched off the rail and made a three mile flight round the ground. It is somewhat late now to describe the sensation of flying which so many thousands have since experienced, but a first flight like this must be unique. I remember wondering whether we should really rise from the rail, and then a feeling of elation when the grass slipped away backwards and downwards and the machine seemed to be sitting on nothing. There was no sense of travelling except by the appearance of the earth moving backwards, and on looking upwards it seemed that we were on a frame structure in a high wind, but without the sense of movement inherent in all other vehicles which are supported below on wheels or on water. In spite of knowing the theory of banking on a turn, I remember marvelling at the machine not slipping inwards when inclined to such an angle. I am afraid, however, that my keenness of observation of the flight sensations was stifled by my greater interest in the man than in the doings of the machine, and the predominant sense was one of wonder that the same man could calmly invent such a mechanism and yet fly it with such consummate skill. Mr. Rolls made his first flight immediately afterwards, then Mr. Butler, and later in the day Major Baden-Powell.

In November of the same year I introduced a party of Englishmen to Wilbur Wright, viz., Mr. Roger Wallace, K.C., Lord Royston, Professor Huntington, Mr. McClean, and Mr. Eustace Short. Having accidentally missed Wilbur in Le Mans, we returned to Paris, where we met him next day, and he entertained us to lunch. Afterwards Mr. Wallace, Professor Huntington, Mr. McClean and Mr. Short returned to Le Mans and were given short flights the following morning, as a compensation for their fruitless journey to Le Mans the previous day. Wilbur never took a fee for any of these passenger flights, and when it is remembered that many people were willing to pay hundreds of pounds for a flight in those early days, these four special flights were a favour of no small character.

During the last three months of the year 1908, when Wilbur beat all his own records and set up new world's records for duration, distance, and altitude, scientists and notabilities came to Le Mans from all parts of the world, many of the former affirming that they had always believed in the flights made by the Wrights in America in 1903 to 1905. Others made numerous suggestions of how to simplify what they regarded as a clumsy apparatus. One distinguished scientist, after looking at the machine for some time, asked Wilbur if he would allow him to measure the camber of the planes, because this would enable him to estimate the length of the undulations followed by the machine in flight. Having been given permission he made a complicated series of calculations and then asked if the length he mentioned was the length of the waves of flight. In reply, Wilbur said that the length of wave was not correct, because the machine flew straight, and not on an undulating path. The scientist was astonished, and replied,

"Impossible," to which Wilbur, ever a pioneer in all he did, replied, "Wait and see." Surely enough the scientist did see the machine make a straight flight soon afterwards, and it is to be hoped that by this time he has forgiven it refusing to follow the laws of the air, which were regarded as infallible at that time.

In January, 1909, Wilbur Wright went to Pau to continue his experiments and to teach the first three pupils how to fly. I was prevented from going by having to undergo an operation, but as soon as I was convalescent early in February, Mr. Rolls gave me a seat in his car and with Mr. Massac Buist we went leisurely across France to Pau, where by that time Wilbur had been joined by his brother Orville and his sister, Miss Katherine Wright. I think it was at this time that I more fully realised how kind and thoughtful these three Americans were, for although kings and world-known men were continually there to see the flights, one or other of those three always found some time to stay by me as I lay on my rug on the flying ground. This was the first opportunity I had had of hearing the two brothers discuss the machine and its doings, and it soon became obvious how suited they were to thrash out the many intricate problems which continually arose. Nothing was ever taken for granted until proved by themselves in actual practice; all the hitherto recognised rules of wind and air currents were forgotten and replaced by new tables in writing contained in little pocket books; and in the arguments, if one brother took one view, the other brother took the opposite view as a matter of course, and the subject was thrashed to pieces until a mutually acceptable result remained. I have often been asked since these pioneer days, "Tell me, Brewer, who was really the originator of those two?" In reply, I used first to say, "I think it was mostly Wilbur," and later, when I came to know Orville better, I said, "The thing could not have been done without Orville." Now when asked, I find I have to say, "I don't know," and I feel the more I think of it that it was only the wonderful combination of these two brothers who devoted their lives together for this common object, that made the discovery of the art of flying possible.

Early in 1909 an American millionaire, with a confidence born of unlimited dollars, came to Pau intending to make a flight with Wilbur, and talking to one of the "English Bunch," he said he had come for that purpose. The Englishman expressed surprise, because as he said he understood Wilbur was not taking passengers, to which the American replied, "Oh! I daresay that can be arranged." "Well," said the Englishman, "I should like to be around when you do the arranging, just to see how it's done." What reply he got to his suggestion will never be known, but he left Pau without having had a ride in the air.

One day we were working together in the little office at Pau, when a card was brought in bearing the name of a lady of title. "That lady, who is an entire stranger," said Wilbur, "wrote saying she was coming here next week, and so as she had settled to come, there was no need for me to reply. Then she telegraphed yesterday to say she would come to-day, and as she had also settled this definitely, there was no necessity for me to reply to this wire. Now she is out in the ground and wants to waste my time without any kind of excuse." With that, the card joined the rest of the correspondence in suspense, and we resumed our work. Later, Wilbur went out and made a pretty flight, which was no doubt viewed by the lady along with the other spectators. I wondered at the time whether it would have been possible for him to gratify everyone, but it was evident after consideration of the circumstances that it would not have been feasible to do so. I also wondered whether the lady herself would be guilty of leaving a letter addressed to her unanswered. I have since been able to satisfy this last speculation, by recently writing to ask her for a small subscription to the Wilbur Wright Memorial Fund, to which letter I have received no reply.

When in Paris, just before the visit of Wilbur and Orville Wright to England, when they received the gold medals of the Aero Club and the Aëronautical Society, and were fêted by both those bodies, I was describing some of the people to Wilbur

whom he would meet in England. Of one I said that he would at once recognise him as being the ugliest man at the Aëronautical Society, and my rudeness was quietly reproved by his replying that the member of the Society would lose that distinction on this occasion because, as he said, "There will then be a pair of us."

Discussing happiness one day, Wilbur remarked that most enjoyment in life consisted of relief from discomfort. To try to be always comfortable and happy was therefore a mistake, for if one succeeded life became unbearably monotonous.

Discussing cricket as compared with baseball, Wilbur described most vividly the excited enthusiasm raised by the American game. His own words in a letter to me of the 9th July last year, from Berlin, make an interesting comparison between German cricket and American baseball:—

> "I would I could go with you—but it seems that it is impossible. A pleasant voyage to you.
>
> "To-day I took a stroll down to Tempelhofen field, where Orville did his flying in Berlin. I was rather surprised to find three or four games of cricket going on in different parts of the field. You will wonder what cricket looks like and sounds like in Dutch, but as I have never really seen a game in England, I do not know how to compare it with the genuine article. While you are in America get Orville to take you to see a baseball game. The conduct of the crowd will interest you immensely whether the game does or not. A baseball game at a school for deaf mutes would be noisy compared with this German crowd at a cricket game."

Ever since I have enjoyed Wilbur's friendship, I have found difficulty in writing the articles on aeronautical and atmospheric subjects that I have been in the habit of doing occasionally for some twenty years past. The reason for this is not that he has in any way abstracted the little knowledge I formerly possessed, but his lucid arguments and unanswerable information gradually altered most of my views, and whenever I have since tried to work out some problem or theory independently I have found myself adopting and arguing some theory of Wilbur's, forgotten for the moment, but recognised when committed to paper. At first I used to mention Wilbur's name in acknowledgment, but after a time I found I had to mention him always, so I ceased to give him credit. This latter course seemed preferable as involving myself alone, and my confession now that I owe everything to him may atone for these omissions.

Describing an adventure by Parmelee on the Mexican border during the last civil war, Wilbur wrote in April, 1911, saying:—

"They were flying over a country where for a hundred miles there was no landing-place except the Rio Grande River. The cut-off cord got wet and shrunk so that it pulled the cut-off handle till one cylinder stopped exploding, so they gradually began to sink towards the river. Parmelee tried to make Lieut. Foulois understand what the trouble was, but could not succeed until too late. Finally just as they were touching the water Foulois "caught on" and pushed the lever back, but the effect was to turn the machine downward a little and make it dive for the bottom of the river. The next thing Parmelee knew he was sitting on the rocks at the bottom of the river with eighteen inches of water over his head. He lost no time in seeking higher atmosphere. Foulois' head soon appeared above water also, but as the lieutenant is only 5 ft. 1 in. tall, and the water was over 4 ft. deep, there was a margin of only about an inch under his chin. The banks were so steep that they had to wade down stream a quarter of a mile before going ashore. The machine was not much injured, and is all right again.

"It was a foolish trip undertaken without proper precautions, and while the cause of the accident was ridiculously trifling, the results might have been very serious."

Wilbur Wright was never an advocate of excessive speed, and in a letter of extraordinary technical interest to the pilot of the Wright machine in the Gordon Bennett of 1911, he said:—

"I had seen from the newspapers that you had finished the course and escaped without any smash-up. These were the only things I really cared about in the race. A matter of a few miles speed more or less was not a serious matter. Your troubles would be amusing if they were not so heart-breaking while they were going on.

"You are quite right in thinking that at high speeds the head resistances become very important, but I see that you are fooled on several points. If you stop to figure up you will find that 80 miles an hour is the speed a falling body attains after a fall of 256 feet. While 40 miles an hour is attained in less than 60 feet. So that a Nieuport cut off at 80 miles and landing at 40 has used up the equivalent of 200 ft. in addition to its actual drop. It ought therefore to go $6 \times 230 = 1380$ ft. before landing, if cut off at 30 ft. height. Your speed the day your motor struck with you was probably not more than 45 miles so you had no surplus energy. A machine with two propellers naturally does not glide as far as with a single propeller.

"Comte de Lambert tells me that the Nieuport motor is 135×145 and runs 1400. Now $\frac{135^2 \times 2 \times 145}{111^2 \times 4 \times 101}$ is in the proportion $\frac{528}{497}$ so that it is a sixteenth larger than our 4-cylinder motor. The speed of the Nieuport with 100 h.p. motor was about what Brookins had with not over 70 h.p. last year. We built that machine to prove that the talk about the speed advantages of the monoplane and the fore and aft arrangements was largely rot. It was faster than any of tne 100 h.p. monoplanes of its time.

"I have written a letter to the *New York Herald* proposing that in 1912 no motor having a total piston area of more than 665 sq. centimetres and 8,000 cubic centimetres displacement be allowed in the Gordon Bennett. This is the size of the 50 h.p. Gnome. If the proposition is accepted we will show them whether the biplane is slower than the monoplane. If they insist on motors of any size the race will not interest us, as we do not care to fly over 100 miles an hour ourselves or put our men on such a job."

The following advice may be useful to young members of this Society:—

"It is always a serious thing to land with a strong wind on the back, because as soon as the speed is as little or less than that of the wind, the pressure on the tail tends to turn the machine over instead of preventing it.

"Therefore I would strongly advise with the little machine, that you always keep sufficient height when going with the wind so that you can turn and land facing it. Young birds often get rolled head over heels by making the mistake of landing with the wind. Old birds never attempt it. It will be well for us to follow their example as far as possible."

It is usual with a genius, for the dominant subject to overshadow all others greatly to their detriment. Wilbur did not suffer in this respect. On subjects of which I had some knowledge he could always add some information, and even in my special work I learnt some points from him. Anyone seeking an example of a clear Patent Specification should read Wright Brothers' original specification No. 6732/04. I cannot say which of the two brothers wrote it, because as I have already pointed out, all their work was so combined, but I can assure the members of this Society that the wording is that of the inventors, and only minor alterations are by the patent attorney. Remembering that this subject was unknown and not taken seriously by practical men at the time, the language is wonderfully clear and the features of the invention are marvellously explained and yet in simple language. Nowadays complicated formulæ are considered necessary to explain

inventions involving but a fraction of the ingenuity displayed in this master Patent.

Discussing the relative costs of Patent actions in America and in England, Wilbur gave me a humorous example of a Patent Attorney's quotation of the probable costs that would be incurred. On the patentee asking how much it would cost to bring an action for infringement against an alleged infringer, the attorney replied by putting the question, "Well, how much have you got?" On the inventor giving an estimate of his realisable assets, the lawyer replied: "Well, that's just the amount the action will cost you." Unfortunately there is a great deal of truth in this fable, and he is a lucky inventor who invents a mechanism that is useful enough to be profitable, but not so exceptionally valuable as to tempt all the world to infringe and to belittle the invention in order to excuse themselves. It would be a poor policy if inventors were refused a just reward for their ingenuity in order to facilitate the free use of their work by all, for the withdrawing of capital and the diverting of intellect from the field of invention into other more secure channels would then result.

When an inventor makes a great invention, he usually sells the foreign rights to the highest bidder. The Wright Brothers had several excellent offers for the British rights, but they did not accept any of these, because they regarded their invention as one carrying moral obligations, and not simply as a negotiable property. They felt it their duty to see that if a company were formed in England, the invention should not be exploited on popular company lines at the risk of the shareholders being left with shares and experience only, in exchange for their money, and as they were unable to withdraw sufficient of their time from their experiments and undertakings in America and on the Continent, they kept the British Patents in their own hands, and have hitherto reaped little profit so far as England is concerned.

This spirit of moderation characterised all their actions. In France rival owners and representatives of other machine makers were welcomed on the flying ground, the machine explained, and in some cases flights given to them. But all their liberality could not save them from the resentment many cherished for the crime of being the first to fly, and so preventing each of those others from being the first if the Wrights had not existed. Fifty years ago men were still on the brink of flight, and with just as much reason, and there is no more justification to expect that these latter men would ever have stepped over the dividing line which their fellow workers of long ago failed to do.

The resentment of this jealousy was afterwards supplemented by the fear of the Wright Brothers endeavouring to collect the tribute which the Crown recognises as the reward of those who give to the nation a new industry, and instead of facing this question in a fair manner many have striven to accuse and excuse, although no application for royalty nor threat of an action for infringement has ever been made against any manufacturer or flyer in Great Britain.

After their return home the Wright Company was formed to work the invention in America, and the companies that had been formed in France and Germany were left to work out their own salvation and to manufacture their own machines. Unfortunately the companies on the Continent were ambitious and missed the guiding hand of the inventors, and many so-called improvements were also introduced into the Continental machines, with the result that while the American company has prospered, the European companies have not progressed. The enthusiasm of this visit to Europe was followed by the natural reaction on the departure of the brothers for America, and the subsequent law actions to enforce the French and German Patents were resented by all who desired to use the invention, but not to pay for it. A campaign of belittlement of the Wrights' invention followed, one claim of anticipation being raised after another, only to be destroyed on investigation being made into its merits. This unfair resistance

to a fair acknowledgment of their Patent rights threw a considerable and worrying work on Wilbur Wright, who was thus drawn against his inclinations from his natural work of experimenting and flying, and his health suffered at times from this uncongenial work. A year before his death I persuaded him to throw up this arduous work in Paris and come to England for a change, and some weeks at Eastchurch leading the simple life and working at Mr. Ogilvie's machine enabled us to send him back to the Continent completely recovered. He was in Boston on similar distasteful law work when he contracted his fatal illness, and if he had not been handicapped by lack of out-door life he would probably have pulled through. As it was, he fought for weeks against the relentless microbe that knew no mercy, and he died as he had lived, a fighter to the end undeterred by heavy odds against him.

Although an American, Wilbur Wright was of English descent, and the son of a family of pioneers. One of the Wrights was one of the early settlers at Springfield, Mass., having emigrated from Kelvedon Hall, Essex, in 1636. A later ancestor moved further West, and was one of the first to cut into the forest and found the city of Dayton, Ohio, and Bishop Wright, the father of Wilbur and Orville, has done pioneer work for the Church, whilst a further brother, evidently imbued with the same pioneer instinct, moved out West to Arkansas before his two brothers became famous.

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